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INTRODUCTORY LECTURE,

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DELIVERED BY

JOHN TAYLOR, M.D.,

Professor of Clinical Medicine in University College, and Physician to University College Hospital.

Lecture II.

It is of consequence that those who cultivate the science of medicine should have decided views respecting the principles upon which such an undertaking must be conducted. Our time will not permit us to enter into an adequate discussion of this subject, but a few remarks upon it will not, I think, be an unsuitable introduction to the proper business of this course of instruction.

The natural sciences are essentially united in all their departments, through all which one spirit reigns, and one method of inquiry applies. (Herschel.) Medicine is one of these sciences: it has been as long cultivated as the rest, and men of the greatest capacities have dedicated themselves to its study, yet, compared with most of them, it is exceedingly imperfect, and some departments of it are but still in their infancy. To what is this difference to be ascribed? Can we reasonably hope to lessen it for the future? I shall offer you a few reasons for thinking it is partly due to the greater inherent difficulties of our subject, which we cannot, of course, alter; and partly to defective methods of investigation, which we may consequently hope to improve. The progress of medicine was retarded, equally with that of the sister sciences, by the errors of the ancient philosophy, as well as by the apathy of the dark ages; but the spirit of the modern philosophy has been less cherished by physicians than by the cultivators of the other sciences, and medicine has profited less by that great innovation than she was undoubtedly entitled to expect. On examining the more perfect of the physical sciences, we shall find that there are three circumstances in which they all agree, and which appear to have had an essential influence upon the rapidity of their progress. 1st. The subjects which they offer for observation are comparatively simple, and can be examined by our senses sometimes directly, often with the aid of instruments of great perfection; the result is, an accumulation of observations possessing a high degree of accuracy. 2nd. Amongst their means of investigation, they admit the use of experiment, which, by disengaging essential from accidental phenomena, and by enabling us to repeat their production at will, adds as much to the pertinency as to the number of such observations. 3rd. The results of observation and experiment admit of being expressed numerically; their exact value can thus be more accurately communicated, and the acquisitions of every succeeding observer admit of being added to the common stock. When we turn to the medical sciences, we observe these circumstances to be attached to each of them in very different degrees, and in each we find there has been a corresponding degree of progress. *Descriptive* anatomy has attained a high degree of perfection, because it has to deal only with simple relations of matter, observable by our unassisted senses, and capable of being re-examined an almost indefinite number of times. *Structural* anatomy is less advanced, because its phenomena are less simple. We require the aid of the microscope, and other means of investigation, by which the sources of error in our observations are multiplied. *Physiology* is more imperfect than anatomy, because it investigates relations and properties of matter much more numerous and complex, and various processes of the living body which are concealed from direct observation, and the explanation of many of which supposes in addition an advanced condition of various branches of natural philosophy. Physiology, however, admits the use of experiment, and, since the extensive adoption by its cultivators of this means of investigation, it has progressed with fivefold rapidity. *Chemistry* is preeminently a science of experiment and quantity; the constant use of measures, weights,

and numbers, gives greater precision to its results, and, compared with some branches of medicine, it is highly advanced, and is still rapidly progressing; but the complexity of its phenomena, and the diversity of causes concerned in their production, and the consequent multiplication of sources of difficulty and of error, have prevented it, as yet, from approaching to the perfection of a few of the higher departments of physics. In the *practice of medicine* we meet with infinitely more complexity and difficulty, and our labour has been proportionately unfruitful. The views I have stated to you may receive support from a slight notice of a few of the perplexities in which the inquirer into this subject is involved. When we attempt to determine the nature and seat of a disease by the symptoms to which it gives rise, we are constantly met by serious difficulties; e. g., the *same* symptoms frequently accompany *different* morbid conditions. *Jaundice* may arise from simple inflammation of the liver, from various structural changes in its substance, and in parts external to it; from obstruction of the biliary ducts by gall-stones, &c. Nearly all the symptoms of inflammation of the peritoneum may arise from hysteria, the peritoneum being perfectly healthy. Paralysis may arise from various alterations in the brain, from injury of the nerves of a part, or it may exist without any appreciable change in either. In persons dying with the symptoms of apoplexy, we may find blood effused into the brain in one case; in another, serum; and in a third, no morbid changes at all. On the other hand, the *same pathological condition* may be manifested by a variety of symptoms; e. g., softening of the brain may be accompanied with coma and other apoplectic symptoms, or it may be attended with convulsions, or with paralysis of motion or sensation, or both. Inflammation of the membranes of the brain may be exhibited in one case by feverishness, acute pain in the head, and violent delirium; in another, there may be scarcely any symptoms which can lead to a suspicion of its existence. Acute inflammation of the pleura, pericardium, or peritoneum, is often accompanied with severe suffering; but I have repeatedly found each of these diseases in the bodies of persons who, during life, complained of no symptoms referrible to them. In the same way, tubercles may exist in the lungs in a very advanced stage, with scarcely any cough or expectoration. Some symptoms are direct objects of sense, and therefore can be pretty accurately observed, such as heat and redness of the surface, paralysis, coma, and convulsions; but many even of these can be simulated. Others are to be learnt only from the description of the patient, such as pain, and various other altered sensations. Our knowledge of these is at best much less exact, and we are often led into gross errors respecting them, through the ignorance, incapacity, or bad faith of patients. There is one department, however, of symptomatology and diagnosis, which strikingly corroborates the principles I am attempting to illustrate, viz., that relating to diseases of the lungs and heart. The introduction of auscultation has given us, in addition to those we had before, a whole class of symptoms which are direct objects of the sense of hearing, and, therefore, appreciable with considerable exactness; the investigation into their immediate causes can be conducted experimentally, and with all the precision attainable in some of the higher branches of physics. We can imitate a great number of them in the dead body, and even out of the body, so as to have the most certain proof of the truth of our conclusions; and the result is, that our knowledge of this class of diseases, instead of being, as it was five-and-twenty years ago, more imperfect than of any other, has attained a degree of certainty and precision which the most sanguine man could not then imagine, and which fully entitles it to be compared with that which we possess of the more exact sciences.

Our investigation into the *causes* of diseases is even more difficult than into the symptoms. That similar causes produce similar effects, is, no doubt, as true in physic as in other things; but we often cannot tell whether, in two given cases, the causes are the same in all important respects. Most diseases are the result of a combination of causes, some of which are known and others unknown, and, in either case, we are seldom able to estimate the respective influence of each. For instance, several persons are exposed to wet, and the exposure is followed in one by sore throat, in another by pleurisy, in a third by rheumatism, and in a fourth by fever. The external and obvious cause is the same in all these cases; but the sum of the causes is, no doubt, very different. There may be differences in the intensity, duration, and mode of application of this common cause; and there are peculiarities of bodily constitution in each case, and other circumstances, the number and character of all which are very imperfectly known, and whose individual influence we are unable to appreciate. We are constantly *misled* by patients—sometimes *intentionally*, as when *moral* causes are concerned, often through ignorance; and frequently they state as a matter of *fact*, what, on closer inquiry, turns out to be no more than a fiction of the imagination, suggested by some crude theory which is current amongst the people.

In *therapeutics*, when we attempt to determine the influence of one or more remedies on the progress and issue of a disease, we reach the climax of complexity and difficulty; the uncertainties of all other branches of medicine are here concentrated and multiplied, and, compared with the inextricable mazes in which this inquiry is often involved, the most imperfect of the physical sciences offers

nothing but a simple foot-worn path. We find, accordingly, that, after the labour of ages, this branch of medicine is yet in its infancy; and our daily experience justifies too well the reflection of a French writer, "les remèdes sont comme les amis: dans le nombre il en est peu qui soient dignes de notre confiance." We shall glance at a few of the difficulties of therapeutics.

1. In estimating the influence of remedies, we must be well able to identify diseases: how many persons have been reported cured of consumption who had suffered only from bronchitis, or some other disease. Suppose a person, when pathology was less understood, to have undertaken a series of researches upon the treatment of dropsy, jaundice, or paralysis, what heterogeneous objects would have been classed under the same name—dropsy from disease of the heart, of the lungs, of the kidneys, of the liver, spleen, peritoneum, &c. &c.; into what absurd conclusions would he not be led: yet our knowledge of pathology and diagnosis are still far too imperfect to secure us against similar mistakes. A most striking example of a false conclusion in therapeutics, chiefly consequent upon a wrong diagnosis, and in a disease usually very easily recognised, is given by M. Chomel. The common holly having been much praised as a remedy for intermittent fever, M. Chomel determined to make trial of it in the clinical wards of the Hospital La Charité. Accordingly he requested the officers of the Bureau Central, an establishment where nearly all the patients are admitted into the Parisian hospitals, to send him some cases of that disease. Twenty-two cases were, in consequence, directed to him. After their admission he purposely abstained from all treatment for several days, in order to assure himself of the correctness of the diagnosis, and to ascertain that the paroxysms were neither suspended nor diminished by the altered circumstances of the patients. He found that of the twenty-two, seven never had another paroxysm, four had paroxysms of decreasing intensity, and eight had nothing but symptomatic paroxysms, connected with slight inflammation of the mucous membranes, which yielded to simple antiphlogistic remedies: three only were fit subjects for experiment, viz., had essential intermittent fever, preserving all its intensity in the three or four paroxysms following their admission. The remedy was given to these, at first, in the dose recommended; the quantity was then doubled, quadrupled, and octupled, without any appreciable effect. The sulphate of quinine was then given in the ordinary way, and the paroxysms were immediately stopped. If from the day of admission, M. Chomel remarks, the remedy had been given to all, or even only to the fourteen who had essential intermittent fever, he would have concluded that it had cured nineteen in twenty-two, or at least eleven in fourteen, and would have regarded the remaining three cases, the only ones proper to test the remedy, as exceptional ones.

2. We calculate the effects of remedies either by their power of shortening the duration of a disease, or of diminishing its mortality. But to estimate this power, it is obviously necessary to know what would be the duration and what the mortality of any given disease if left to nature; e. g., mercurial ointment has been much praised in the treatment of erysipelas, but M. Velpeau has attempted to prove that the duration of the disease, when treated by this remedy, is precisely the same as when nothing is done, or when the part is simply anointed with lard, and, consequently, that it is useless. We are almost destitute of this kind of knowledge of diseases, and we are likely long to remain so, for few patients would choose to be left to the undisturbed powers of nature, with the view of enabling their physician to observe the natural course of their disease. Many of the cures attributed to homœopathy are wholly due to the efforts of nature: in such cases the physician has simply been a harmless spectator.

3. The duration and mortality of the same disease vary much with the age, sex, and previous health of the patient, and with its complications in different cases. For instance, pneumonia is much more fatal in infancy and old age, than in early adult life; and in persons previously broken down in health, than in such as are vigorous up to the moment of attack. Louis states that pleurisy attacking persons previously in good health, always, or almost always, terminates favourably; and he has never known death to result from erysipelas of the face, gastritis or enteritis occurring in the same circumstances. In this case we may ascribe to our treatment a favourable result, which, in reality, is due to the fortunate age or constitution of the patient.

4. The state of a patient's mind powerfully influences the progress and issue of his disease, as well as the physiological effects of remedies. We are often baffled in the treatment of a disease produced by excessive anxiety, his circumstances alter, the patient gets well, and we put down the cure to our remedies. I have known a dose of laudanum purge a female, who supposed she had been taking an aperient draught. The state of patients' minds is seldom well known to us, and often carefully concealed from us, and, of course, its influence can only be vaguely allowed for. Without considerable precaution, the peculiar dispositions of some patients may lead us into error, in estimating the effects of remedies: many, when questioned, invariably declare themselves better, and continue to do so even to the day of their death; others, regularly and resolutely, refuse to acknowledge that they have experienced any improvement. They cling to their disease as

if they esteemed it a benefit, and the expression of any anticipation of improvement appears to be received almost as an attempt to do them some serious injury.

5. Most persons, when sick, especially in hospitals, are subjected to changes in their diet, lodging, clothing, and in their general habits: these circumstances alone will cure many diseases, particularly chronic ones, and our medicines often get the credit which is really due to them.

6. Several remedies being commonly used at the same time, it is difficult to judge what is due to each, and how much to the co-operation of several; yet nothing is more common in such a case, than to find a favourable result unhesitatingly ascribed to some one of the number.

7. Other difficulties, and those not small ones, arise from the adulteration of medicines. This abominable practice is so systematically and extensively carried on, that we can seldom feel assured that the drugs we prescribe are received by our patients in any tolerable purity.

8. Again, a remedy may seem useless, because the dose has been inadequate, or it may not have been persisted in for a sufficient length of time. Its good effects may be equally missed if we are obliged to discontinue it, from having given it at first in too large a dose.

9. The substances we direct are often not at all, or are improperly administered—sometimes designedly, sometimes from negligence or ignorance; in any case the fact is usually concealed from us; and if the patient get well, we take no less credit for the result. Velpeau mentions the case of a boy admitted into one of the hospitals of Paris for ophthalmia. The surgeon, in order to assure himself of the progress of the disease, and thereby of the efficacy of his treatment, did nothing the first day. On the second day the disease was worse, still nothing was done. On the third there was a further aggravation, and a solution of nitrate of silver was ordered to be dropped into the eye. At the next visit there was a great improvement, which produced a corresponding conviction of the value of the remedy. No case could apparently be clearer: it turned out, however, that through some error the remedy had never been applied. Nothing is more common than such occurrences in private practice; and capital jokes are daily told us which derive all their zest from the custom, upon which some persons appear to plume themselves, of making any use of medicines rather than that which the practitioner contemplated.

Such are examples of the complexity and difficulty inseparable from the nature of many of the problems which the physician must attempt to solve. These circumstances are sufficient to explain much of the difference to be found in the state of our knowledge of medicine, and of some parts of natural philosophy; but they are not the sole causes of this result. We shall glance for a moment at the methods which appear necessary in the solution of our difficulties, and, by comparing them with those generally adopted, we may see reason to believe that a few persons only, and that very recently, have been fully imbued with the spirit of the processes, by the use of which alone we can hope to arrive at truth.

* In entering upon the study of any disease, we must first make a collection of observations relating to it, and every possible precaution should be taken to secure their accuracy and completeness. The least reflection on the difficulty of ascertaining what are facts, and of collecting them exactly, as well as on the number necessary to settle any question in science, will satisfy any competent person that the work of observation is most difficult—that it demands an almost exclusive devotion to it, and that long practice is necessary to excel in it. Yet this fundamental labour has been frequently confided to pupils, almost without experience, and who, however intelligent, could not be sensible of the importance of the work which was intrusted to them. "We should laugh at the chemist," says Louis, "who got his analyses made by those just entering upon their career; what, then, can we think of physicians who intrust the collection of facts to their young pupils?"

This collection should include all cases *indiscriminately*, and not merely *extraordinary* ones. The latter practice has very commonly been adopted, and has made us acquainted with *one* aspect of the disease only, and that usually the most unfavourable: for instance, structural diseases of the heart have been thought to be alarmingly rapid and fatal, because until lately we have been familiar with them only in their advanced stages. More accurate investigation has now taught us that they are exceedingly common, and may often continue for years with comparatively little inconvenience to the subjects of them, and before reaching that stage with which we were previously chiefly acquainted.

All the *functions* must be examined during life, and *all* the *organs* after death; *negative* as well as *positive* facts must be inquired into; and all these are to be *recorded*, not trusted to the memory. Instead of this we have almost universally contented ourselves with short histories of cases,—the leading features only have been delineated, and after death those organs alone which were previously supposed to be diseased have been examined. Such accounts are far too incomplete to supply materials for descrip-

* In sketching the following outline of the numerical method, a free use has been made of Louis's Essay on Clinical Instruction.

tions which make any pretensions to accuracy, for in most diseases we find various symptoms and lesions, more or less important, in addition to those which belong directly to the part primitively affected. Without this examination of *all* the functions during life, and of *all* the organs after death, how could we ascertain that very serious lesions may exist with very trifling symptoms, or with none at all? Every one in the habit of examining a large number of dead bodies must be constantly meeting with extensive morbid changes in organs, in which until then no disease had been suspected. This observation applies to a variety of diseases of the brain, as effusion of serum into its cavities, softening, and tumours of various kinds developed within its substance. It is equally true of many serious lesions of the heart, lungs, liver, stomach, and kidneys, and, indeed, I believe, of every organ of the body. A knowledge of the fact, that various diseases are found more or less constantly connected with each other, and that the one is very often the consequence of the other, could not have been arrived at by any other means. I have never seen a case of diabetes mellitus in which phthisis did not supervene during its course. The liability of inflammation of the pericardium and of the lining membrane of the heart to coexist with acute rheumatism, is a well-known instance referrible to the same class: pleurisy is very apt to occur in the same circumstances; and pericarditis is a frequent complication of pleurisy and of pleuro-pneumonia. I may likewise instance the constant increased vascularity of the mucous membranes of the stomach, intestines, and lungs, whenever there has been any febrile excitement; the occurrence of pneumonia in fever, and in other prostrating diseases; and of bronchitis and emphysema of the lungs in rather advanced disease of the heart.

Having collected a sufficient number of such facts, we are prepared to describe the disease and to determine some of the laws to which its phenomena are subjected. In this work we should remember that we are not to *guess* the truth, but to *find* it; and that if we affirm anything more than is deduced from a rigorous analysis of our facts, we are very probably dictating to nature a language she will disavow, and we very certainly advance an assertion without proof. (Louis.) If we wish to know the symptoms of a disease we must not refer to our memories, and state vaguely that such and such were frequent or rare, because the symptoms most prominently registered *there* will not be those which occurred most frequently, but those which from their singularity or other causes made the strongest impression. We must search through our written facts—we must count how many times such symptoms have been noticed, and state the result in figures. How else can our experience be understood by others? how added to their own? "Who knows the value of the expressions more, less, rare, frequent? Do we mean by frequent, twenty, thirty, fifty, or sixty times in a hundred?" But in investigating the symptoms of a disease, it is not enough to count the number of times which each was observed in that disease alone. In order to fix the value of a symptom, we must ascertain whether it is *peculiar* to the disease under consideration, or its relative frequency, time of commencement, duration, &c., in that disease, and in others of the same class: e. g., a dry and red appearance of the tongue frequently occurs in inflammation of the mucous membrane of the stomach, and hence has been very generally considered to indicate the existence of that disease; but a comparison of the state of the tongue in gastritis and in other diseases, has shown that the tongue may be dry and intensely red when the gastric mucous membrane is healthy; and also that if it be sometimes red in gastritis, it is still more frequently pale. Hæmoptysis, again, was noticed in two-thirds of the cases of phthisis examined by Louis, or in fifty-seven out of eighty-seven, from which we should infer that it is a symptom of some importance in that disease; but how much greater does its value appear, when we learn that amongst twelve hundred patients (not including cases of amenorrhœa, or those arising from external violence), not one besides those labouring under phthisis had experienced hæmoptysis. Delirium in fever has been regarded as a sign of disease of the brain; Louis, however, found, on comparing the state of the brain in twelve cases, in which there had been the most violent delirium, with its state in twelve where there had been no delirium whatever, that the morbid appearances were exactly the same in the two series, and that that organ was perfectly healthy in about one-half the number of each. By a similar comparison the same observer has proved, that the delirium is equally independent of any morbid condition of the stomach. Thus, the study of the symptoms of one disease implies the careful observation and comparison of many others. The work of Louis on typhoid fever is based on an examination of no more than one hundred and thirty-eight cases of fever; but, on the principle above stated, in order to establish the history of the disease he was studying, he made a careful analysis of about nine hundred cases of acute disease generally.

The same process is to be repeated in determining the *pathological anatomy*. For instance, typhoid fever has been maintained to be a gastro-enteritis. If this opinion be true, we ought to find the stomach much more frequently affected in those who have been its victims than after death from other diseases, and always inflamed when it has

been rapidly fatal. M. Louis has examined this question, and found that the stomach was perfectly sound in the individuals *rapidly* carried off by this disease; and in the rest it was not more frequently inflamed than after death from the most varied acute febrile diseases. This argument seems quite decisive, but could never have been arrived at without *counting*, and this counting would have been of little value without the previous observation of the state of *all* the organs, of the *negative* as well as *positive* facts; and that not in the disease under consideration only, but in a great variety of other diseases necessary to be compared with it. Various important general facts in pathological anatomy could never have been suspected, much less established, by any other method. The following are instances of such: Louis, in the examination of a great number of cases, has never met with tubercles in any organ after the age of fifteen, without, also, finding them, at the same time, and usually in a more advanced stage, in the lungs. The same observer has found that ulcerations of the pharynx, œsophagus, and small intestine—if we except syphilis—are peculiar to two affections, viz., fever and phthisis; that ulcerations of the larynx and trachea (especially the latter), with the already-mentioned exception, are found only in phthisis, and the fatty degeneration of the liver is seldom met with in any other disease. Again, after the age of fifteen, peritonitis, which is chronic from its commencement, is always dependent upon tubercles developed upon or beneath the peritoneum; but, according to a previously-mentioned law, tubercles in any organ after this age imply the coexistence of tubercles in the lungs: hence a chronic peritonitis may lead us to the diagnosis of phthisis independently of any symptoms in the chest, and even in the absence of them. In the same way, Dr. Clendinning has come to the conclusion, that the average weight of all the visceral organs of the body is greater in persons who have died with disease of the heart than in any others; consequently, that diseases of the heart have a tendency to induce hypertrophy of the other organs. I do not vouch for the accuracy of these results, though I have no reason to doubt it, and much to believe in it; but, if true, they are unquestionably important, and their truth or error can only be established by always examining the state of *every organ*, by collecting all facts *indiscriminately*, and by *counting* them.

In the study of *causes*, we have no other method of proceeding. If, amongst the circumstances immediately preceding the disease, one be found to have occurred in ten cases out of a hundred, we may attach little value to it, but the case is very different if it have been observed eighty times, and especially if it be found to be a less frequent antecedent of other diseases; for in the appreciation of causes, as in that of symptoms and morbid appearances, it is always necessary to institute a comparison between the affection we are studying and a number of others. In this way if we do not always arrive at a positive we may come to a negative conclusion; we may ascertain what is not, if we fail to find out what is the cause. If it be found, for instance, that pulmonary catarrh and pneumonia are more frequent in males than in females in the proportion of three to two, and that the contrary is true of phthisis, such a fact must be of great weight in the discussion of the doctrine that phthisis is always produced by bronchitis or pneumonia. Cancer is maintained by many to be a consequence of inflammation; but from the examination of a great number of bodies, it appears that whilst inflammation is most common in the lungs and intestines, cancer is found most frequently in the uterus and stomach, next in the liver, and afterwards—but separated by a long interval—in the lungs and kidneys. Out of eight hundred subjects whose viscera were carefully examined, there was not found a single example of cancer of the small intestine.

In *therapeutics*, where the difficulties we meet with increase, and where a greater number of facts are required to surmount them, it is still more necessary to *register*, and to *count* these facts. All men in maintaining the superiority of one treatment to another, appeal to their experience for proof: some count their facts and state their numbers exactly; others trust these numbers to their memories at the risk of altering them unwillingly; but all proclaim the superiority of their favourite treatment, because they have seen it more frequently and more rapidly successful: both parties adopt the mean of a number of trials as the measure of the influence of the treatment they support. In a question, then, of so much importance, is it not better to be guided by recorded figures whose exactness is certain, than by our simple recollection which so many circumstances may completely vitiate? It would be very extraordinary if a conclusion, legitimately deduced from observations confided to the memory, should cease to be valid, by the simple fact of these observations being arranged in columns, and added together on paper.—(Gavarret, Principes Generaux de Statistique Medicale.) Some of the more complex questions in therapeutics require for their solution so great a number of facts, that no memory could be trusted to retain them; besides the *treatment* adopted, so many other circumstances, as we have already seen, may influence the issue of the disease, that no one can estimate the influence of each in a single case, or in a small number of cases. If we take a large and an equal number of cases treated on each of two different plans, the circumstances alluded to will sometimes operate against, and some-

times in favour of, each of the two plans, and will neutralise the influence of each other; and if we find the average result under one plan of treatment much more favourable than under the other, we may fairly adopt the conclusion, that this difference is chiefly due to the treatment, and, consequently, that the former is the more worthy of our confidence. In questions requiring for their solution a much *smaller* number of facts, experience has fully proved that we cannot safely trust them to our memories. "The mind," says Laplace, "has its illusions, like the sense of sight; and as the touch corrects the latter, so reflection and calculation correct the former. A degree of probability, founded upon daily experience, or exaggerated by fear and by hope, strikes us with more force than a higher degree of probability, which is only the simple result of calculation. Thus, for slight advantages, we do not fear to expose our life to dangers much less improbable than the issue of a *quine* from the lottery of France; yet no one would be willing to obtain the same advantages with the certainty of losing his life if this *quine* turned up. Our passions, prejudices, and dominant opinions, by exaggerating probabilities favourable to them, and by weakening the contrary ones, are abundant sources of dangerous illusions. *Present evils*, and the cause which produces them, affect us much more than the *recollection* of evils produced by a contrary cause; they disqualify us from appreciating justly the inconveniences of the one and of the other, as well as the probability of the means proper to preserve us from them. * * This lively impression which we receive from the presence of events, and which scarcely permits us to remark the contrary events observed by others, is a leading cause of error against which we cannot be too much on our guard." Hence the agreement of some remarkable events with the predictions of astrologers, soothsayers, and augurers, likewise with presentiments and dreams; we do not reflect on the great number of non-coincidences which have made no impression, and which we are unacquainted with. "Thus," he adds, "the philosopher of antiquity, to whom was shown in a temple to exalt the power of the deity who was adored there the ex voto's of all those who, after having invoked him, had been saved from shipwreck, made a remark conformable to the calculation of probabilities, in observing, that he did not see inscribed the names of those who, in spite of this invocation, had perished."—(Essai Philosophique sur le Calcul des Probabilités.) Medicine generally, and therapeutics in particular, abounds in illustrations of the same principle. Prejudices, from the influence of which it is impossible to escape, constantly warp our judgments of the effects of remedies; some are prescribed on *hypothetical* grounds, and we are disposed to see only the results we anticipated, and to take no account of the opposing facts; the utility of others is denied, not from a candid appeal to experience having proved unfavourable to them, but because we cannot comprehend in what way their good effects are produced. We dictate to nature when we ought simply to observe her; so that if, according to our *notions*, a remedy *ought not* to be useful, the results of observation will also point too frequently to that conclusion. In extreme cases men will deny the evidence of their senses when it is opposed to a favourite idea; and some might, perhaps, be found to adopt the answer, imputed to a Frenchman, who, on being told that the appearances of nature were opposed to some favourite theory which he had espoused, complacently declared that nature, then, must certainly be wrong. On a point of such importance, however, it will be better to appeal to facts. Laennec boasted that, by the use of tartar emetic in large doses, he had almost annihilated the mortality of pneumonia. Well, the records of the hospital have been lately appealed to; his cases have been examined; his facts have been counted, and the result is found to be that he was not more fortunate than others, having lost two cases out of every five: "thus it was with Laennec, as with so many others, two successes effaced one reverse;" that event appeared to be the most *frequent*, which, from the unconscious preoccupation of his mind, had merely made the *strongest impression* there. Louis states that, when he came to a conclusion from the simple recollection of facts which he had not analysed, he found, in *every instance* after this analysis was made, that his previous conclusions were false. To the same source is to be ascribed the successive rise and disappearance of fashionable remedies, the reputation of homœopathists and of empirics, and the contradictory opinions which disgrace our therapeutics. "The time," however, "is come for rendering justice to medicines of usurped reputation, and for placing the character of really efficacious ones upon a more solid basis; this is the noble mission of clinical medicine, which, by effecting this simple purification, may attain one of the most valuable objects."—(Forget.) The mode of studying disease which I have thus imperfectly noticed, is generally known as the "numerical method" of M. Louis; not that Louis is the inventor of the method, for it is no other than that of Bacon, so long in general use in the other physical sciences; but to him, far above any other man, is due the merit of insisting on the necessity of a systematic employment of it in medicine, of demonstrating its capabilities, of suggesting some of its important details, and, above all, of exemplifying its application by the publication of works, which, as models of labour, patience, and self-denial, directed by enlightened views in medical philosophy, and by a scrupulous regard

for truth, have, perhaps, scarcely been rivalled in the history of medicine, and certainly never surpassed. I cannot sufficiently regret that my attention was not early directed to these admirable productions; and if my strong recommendation should lead you to their careful study and imitation, you will hereafter be willing to acknowledge that I have rendered you a signal service.

If the preceding remarks be true, it must be evident that the difficulties of medicine have not yet been fairly met: instead of the facts weighed and counted which are now demanded, what do we find in the records of our art? Vain echoes; authority speaking in the name of authority; scattered facts loosely observed and selected for their singularity; opinions deduced from unrecorded observations, and without appreciation of the circumstances which alone could give them weight. Very great results, I think, may be fairly expected from a more cordial recognition and more general adoption of the numerical method. So entire is my conviction of the inadequacy of the methods till now *chiefly* in use, and of the vastly superior capabilities of this, that I am almost ready to adopt the strong language of one of its early supporters, "We feel convinced that the labours of one hundred medical men, strictly undertaken on the principles we have advocated during a period of twelve months, would do more for the elucidation of many of our difficulties, than the uncertain materials of the last two thousand years." — (Dr. Cowan's Introduction to Translation of Louis on Phthisis.)

When we look back, then, on what has been accomplished, and compare it with what remains to be done, we cannot avoid the conclusion, that we are now executing the labour by which future generations are to profit; ours is the age for observation, for accumulating the materials by a rigorous analysis, of which the existing defects of the science of medicine must hereafter be made to disappear.

Before concluding, Gentlemen, I beg to make one or two remarks on the mode in which the hospital may be advantageously made use of. The hospital practice, I think, ought to be attended both by the junior and the senior students, but by each class in a different way and with different objects. The time of the junior student is much occupied in the study of elementary subjects; these are enough to engage all his serious attention, and he is, therefore, unable and ought not to attempt to attend *systematically* to the practical study of disease. To endeavour to do this would involve him in a double error; he must first neglect the study of preliminary subjects, which it is important for him to master; and next, for want of this kind of information, he would be unable to avail himself fully of the advantages which the hospital affords; but the junior student has many fragments of time unemployed in the intervals of lectures and of dissection, which might be profitably spent in the hospital in acquiring a familiarity with such elementary phenomena in diseases, as require for their appreciation neither much previous study nor subsequent reflection. For instance, walking into the medical wards, he may see various cutaneous diseases; and without troubling himself about their causes, treatment, or other circumstances, unless such information fall directly in his way, he may in a few minutes learn all that is essential about the *diagnosis* of some forms of them. The characteristic crusts of porrigo favosa once seen and explained, can hardly be forgotten again; in the same way he may soon learn the leading external characters of lepra, erysipelas, and many other forms of disease; and this information will be found very useful both during his attendance upon the lectures on medicine, and when he afterwards comes to devote more time to the hospital. The senior students, however, should make the hospital practice their leading object; those who make up their minds to attend to it at all should do so in good earnest; they should not follow one physician for two or three days only, and then go with a second and with a third for a similarly brief period, and afterwards absent themselves altogether for some time. Perhaps they will find the practice of one, or, at most, of two physicians, or of a physician and a surgeon, enough to occupy their attention at one time, and, after attending regularly with one during a reasonable period, they may leave him to follow another, so as to have the advantage of seeing the practice which is peculiar as well as that which is common to each. I would advise such students to select certain cases, for they will probably find the whole too many to attend to at first, and to endeavour to place themselves as much as possible in the situation of the physician in charge of them; to investigate carefully their history; to form their own diagnosis, and compare it with that of the physician; to watch the progress of these cases regularly from first to last with the same care, and to make full notes of all that they observe. The practice of note-taking leads to greater care and accuracy in observing, and, as we have just seen, it is the only way by which experience of any value can be accumulated. To do all this, however, will require much time and attention; many, I believe, have begun such a system with the best intentions, but have quickly fallen off, because they have not found all the profit they expected. These gentlemen have missed their object for want of perseverance; there are many difficulties and discouragements in the threshold of practice, but those who will resolve to surmount them will be well repaid for their trouble. If they once go far enough to feel the advantages of such a course, it will then be as difficult to in-

duce them to quit it as it is now, too often, to persuade them to enter upon it.

With the view of exciting greater and more regular attention to the observation of disease, and also of ascertaining what progress the student is making, it is proposed that I shall hold one examination weekly on the cases under my care, and that I shall likewise put questions at the bedside during the visits; I shall, therefore, be glad if those gentlemen who are willing to submit to these examinations will favour me with their names, that I may place them in a list to be kept for that purpose. The highest inducement I can hold out to any one to take the trouble to prepare himself for such examinations, is the valuable information he must acquire by this increased attention to the book of nature; but if an additional motive should, in any instance, be required, I may state, that as I shall have much better opportunities of ascertaining the amount of practical knowledge possessed by the gentlemen who submit to examination than by those who do not, I shall be enabled on any future occasion to give certificates to the former of correspondingly greater value.

EXPOSITION OF A METHOD EMPLOYED FOR THE

CURE OF CONSUMPTION,

BY FRICTIONS WITH

OLEUM ANIMALE FŒTIDUM,

CONNECTED, AS REGARDS THE PRINCIPLES, WITH THE "LARD-CURE" AND "INHALATIONS."

By Ulric Palmedo, M.D., of Berlin.*

As to their origin, all complications of consumption may be distinguished into two classes.

A. In such where the pulmonary affection is in a casual conjunction with another disease, where, in consequence, no casual connection exists between both maladies, but, on the contrary, each proceeds from a distinctly different cause. (Real complications.)

B. In such where there is a certain genetic connection between them, and the production of the one is the natural sequence of the existence of the other, or of its cause. (Compositions.) These, again, can be subdivided under three heads—1, where both affections can be deduced from the same cause, which is the general tuberculous dyscrasia; 2, where phthisis is the consequence of the other disease; or, 3, on the contrary, where phthisis is the cause of the other.

This division of the complications as to their genesis, though it is unconstrained and natural, and although the diagnosis which refers to it is so just and proper for the general study of consumptive diseases, does not always afford the practical advantage which at first sight it appears to promise: for, in the first place, we find it often very difficult, sometimes impossible, to inquire into the origin of such complications; and, secondly, the possibility of curing consumption, and the choice of the treatment do not unconditionally and exclusively depend on this origin of the complicated disease. Nevertheless, we ought not to neglect this genetic investigation, as it often may facilitate our forming a just conclusion as to the prognostic of the disease.

To arrive, however, at a just prognostic in cases of complicated consumption, the genetic investigation is less important than the diagnosis of the actual nature and character of the complicating disease; how far, namely, the latter is still dependent on the phthisis, or whether it has already acquired a substantial and independent character of its own, whether it is curable or not, whether it has a great or a small influence on the general constitution, and on the lungs in particular; and, lastly, how far it favours, or discountenances, or totally hinders those changes which are sought to be effected in the lungs by the frictions with the oleum animale fœtidum.

On the nature of these relations and proportions entirely depends the importance and significancy of any complications in consumption; and the particular case must be judged with a necessary reference to them, if a just prognosis and choice of treatment is the object to be attained.

a. Disorders of the larynx and the windpipe accompany pulmonary phthisis so frequently, that we may almost assert that they are never absent; the voice, at least in the later periods of consumption, is almost always changed, dull, damp, and hoarse; and an increased sensibility of the larynx or the trachea, difficulties of deglutition, as well as the other symptoms of a laryngean affection, generally make their appearance at that time. Besides, organic changes at these parts are so frequently observed in bodies of persons who died of pulmonary consumption, that they are scarcely missing in one case out of three. Nearly equally numerous are the varieties of these changes and of their symptoms. But the diagnosis of these various states is unfortunately imperfect and uncertain. Whilst a small inflammation, or a simple irritation, may sometimes occasion a violent cough and many accidents, not unfrequently the greatest ulcers have been developed, and followed by incurable destructions, without having been accompanied by any considerable increase of the cough, or by any other important molestation; and again, a small superficial ulcer on another not very remote spot of the larynx may cause the most violent cough, a complete loss of voice, and suffocation: yet, on account of the important difference in the prognosis of phthisis pulmonalis and phthisis laryngea, an accurate diagnostic would be of momentous consequence. For the genuine

* Continued from p. 86.

phthisis laryngea being, according to all experience, an incurable disease, in spite of its protracted course, and even the treatment with the animal fœtid oil having been at the utmost capable of relieving it merely for a time, we must conjecture an equal unsuccessful result in those cases where there exists a conjunction of this *genuine* laryngean disease and of the pulmonary phthisis. But such a tuberculous affection of the larynx may generally be inferred only to exist, when the laryngean symptoms preceded the origin of the pulmonary complaint, or accompanied it from its commencement. There are, indeed, instances where the true laryngean phthisis develops itself, but during the later progress of the pulmonic one. But this seldom takes place in this laryngean disease, which itself we know to be but of a rare occurrence, in comparison with the frequent appearances of other affections of the larynx, which have no proper and independent origin, but are simply the immediate consequences of pulmonary consumption.

In several cases of a perfect cure of pulmonary phthisis by my method, the larynx was considerably affected; this was indubitably indicated by a painful sensation in that organ, a continual hawking and effort to force up phlegm, a frequent and convulsive cough, a changed and enfeebled voice, and a difficulty of deglutition. Such a consensual affection in the larynx and trachea, which only appears in the later periods of pulmonary consumption, is, in fact, a very common and almost constant phenomenon. This consent is partly caused by the general irritation of the lungs, and likewise and chiefly by the frequency and violence of the cough, as also by the irritating quality of the sputa,* and appears to consist in a mere superficial irritation, inflammation, or ulceration of the mucous membrane; whilst, on the contrary, the immense destructions in a true phthisis laryngea have their original seat in the submucous tissue, and in the deeper structures of the larynx. The doctrine, however, of the tubercles of the larynx is greatly behindhand, if compared to that of the kindred phthisis pulmonalis. As long as such laryngean affections, the consequence of pulmonary consumption, have not attained an absolute incurable degree, they cannot hinder the convalescence of a consumptive patient, whose pulmonary disease we are enabled to remove: they, at the utmost, may render the cure somewhat more difficult and tardy; and it is by no means advisable to abandon hope too soon, and to forfeit the chance of success.

b. Bronchitis, pneumonia, and *pleuritis* occur, we know, frequently as complicating diseases of tubercular phthisis, and may then exist in various connection with it; viz., as cause, effect, coeffect, or accidental conjunction. But with regard to the prognosis of pulmonary consumption, they themselves are of no great consideration, because they are, in general, easily removed; so much the greater, on the contrary, is, after their removal, the importance of their results, their issue and consequences, for these form the principal and most frequent impediments to the progress or the success of a cure of consumption.

Bronchitis, although one of the most frequent affections which precede or accompany the first formation of pulmonic tubercles, is also, though much more seldom, the supervening remedy which nature introduces to expand the air-cells, and thereby to effect the cure of phthisis: and so far we may rather consider bronchitis in general as a favourable circumstance assisting the remedial qualities of the animal fœtid oil. Sometimes, however,—and it happens in very chronic cases,—the remaining chronic bronchitis is a state which, though not dangerous, still continues refractory long after the removal of the pulmonary affection, requires a peculiar treatment, after the principal cure by means of the fœtid oil has been perfected, and, therefore, does not allow complete convalescence to take place as early as in more acute cases.

Pneumonia, which, as often as bronchitis, precedes the formation of pulmonary tubercles, also not unfrequently makes its appearance during the course of tubercular consumption, which then is either its cause or accidentally joined with it; and hence principally arise the frequent and repeated periodical aggravations which we observe in consumptive patients. They often mark distinctly the transition from one stage to another; even if fortunately overcome, they may still contribute to accelerate the fatal issue of the primary disease. They have been the frequent cause of failure in my experiments with the remedy of the animal fœtid oil: for, besides that every violent inflammatory irritation is a contra-indication which prohibits the use of this oil, the condensation of the pulmonic parenchyma, if existing to a too considerable extent, is the most unfavourable circumstance towards suppressing the tubercles already formed. Yet this condensation is the most frequent effect of pneumonia, continually accompanies pulmonary tubercles, and unceasingly

* The influence which the expectorated matter has, on the formation of ulcers, in the windpipe and the larynx, becomes more than probable by the following observations:—"In very many cases where there was a phthisical abscess in the lungs, I have observed the existence of ulcers advancing towards the bifurcation of the trachea. They were traceable to the first division of the bronchus, but were thence only found in one tube, which in all cases was that which led to the excavation in the lung."—Retranslated from the German translation of W. Stoke's Treatise on the Diagnosis and Treatment of Diseases of the Chest, p. 374. Bremen, 1838.

favours the formation of new ones; for it diminishes, without intermission, that mass of the lungs which is still serviceable to respiration, and particularly where it exists in the vicinity of tubercles and their excavations, it opposes an insuperable impediment to the efficacy of the oil, by rendering impossible the enlargement of the vicinal and impervious air-cells. This, unfortunately, took place several times in those cases of my practice where the fever lasted the whole day without intermission, and with an almost equal violence, and where the respiration was short and accelerated, the cough frequent, short, and dry, and the expectoration very scanty, during the whole course of the disease, consisting almost entirely of transparent phlegm (phthisis sicca). The sanguineous depletions which were indicated by these symptoms and by the stethoscopic signs, and which were often repeated, produced but a small change, and that of short duration only. When the animal fœtid oil was employed, it was also without result, and failed in its peculiar effect on respiration. If, on the contrary, the condensation is not too considerable, the obstructed air-cells gradually become again pervious to the air by the increased efforts of inspiration, and ultimately attain their full and beneficial dilatation, as soon, namely, as hepatisation has been gradually removed by this very process, of a partly mechanical and partly dynamical nature: small venesections and the use of dissolvents will materially assist this process.

Similar to the influence just described, which the consequences of pneumony have on pulmonary phthisis, is that of the effects of *pleuritis*, as exudations and adhesions; by these, namely, the healthy and still unaffected portions of the lungs are impeded in their regular action, and prevented moving and extending properly, and effecting the compression and extinction of tubercles and excavations. But too frequently the consequences of pneumony and pleuritis occur simultaneously, and sometimes to a considerable extent and high degree: it is, in general, not difficult to discover their existence by the physical exploration of the chest.

The particular conditions under which, in this complication, the animal fœtid oil may completely develop its remedial powers, are, therefore, that a still considerable portion of the chest gives a clear sound on percussion; that auscultation discloses a distinct respiratory murmur, joined at the utmost with some phlegmy rattle, but with no other morbid sounds, and audible in a not too limited space, where the voice also must be clear and sonorous; and, lastly, that the thorax during respiration possess its proper movement. The more these requisites are met with, the quicker, more complete, and striking will be the success: under opposite circumstances, the reverse will take place, or there may even be a failure of the cure.

C. Next to the diseases mentioned under the sections *a* and *b*, *organic diseases of the heart* form the most frequent complications of pulmonary consumption. Relying on their anatomical researches, some physicians assert that they have found, at least, one defect in the heart in every six cases of phthisis. This statement is hardly surprising, if we consider the frequency of this evil even in persons free from consumption; as also the influence which the derangement caused by diseased lungs in the circulation of the blood must have on the organisation of the heart. Still more frequently, and perhaps with a greater certainty than by dissections, we have the means of proving already during life the existence of irregularities of this organ, which are either too small to be recognised after death, when the vital powers of the heart no longer assists in their discovery, or are of a mere dynamic sort. It is a peculiar merit of auscultation to have enlarged our diagnostic powers, by which the existence of affections of the heart is now so much oftener disclosed. If, on the one hand, this recent knowledge, so amplified in comparison to our former unchary ignorance, is of a dejecting kind, because we almost to a certainty discern in these cardiac defects the germ of an early end in so many persons, "hæret lateri lethalis arundo;" yet, on the other hand, an extraordinary advantage is so far gained, that we are thus timely enabled, by removing much of what could be detrimental, at least to impede the progress of the malady, and consequently to prolong life.

The diseases of the heart which occur in phthisis are to be considered either as occasioning the pulmonary tubercles, or as their effects: the latter is by far the more common case. To decide on the priority of these two diseases when coexisting is often very difficult, if the origin of the disorder of the heart has not been observed to have taken place during the course of the phthisis, or, which is frequently the case, if distinct anapunestic signs are wanting, and no account can be furnished of anterior circumstances. A solution, however, of this question is sometimes offered by the species as well as the seat of the cardiac disease. Thus, an imperfect closure of the communication which in the fœtus exists between the two halves of the heart, is in some rare cases a cause of pulmonary phthisis (phthisis cyanotica). The affection of the heart must likewise be declared to be a primary one, or at least to be independent of pulmonary consumption, when we have discovered that its essence consists in a defect in the valves, or in an enlargement of the left heart, be it with or without hypertrophy.

On the contrary, enlargements with hypertrophia of the cavities of the right heart,

when coexisting with phthisis, may be consequences as well as causes of the latter disease; for the blood, on account of its disturbed circulation in the lungs, being held back in the right heart, distends the less resisting parietes of this half: and in addition to this mechanical dilatation of the right ventricle and auricle, the blood also acts in an exciting or stimulating way upon these cavities. In consequence of this stimulus, the process of the nutrition of the heart tends to become more active, and therefore causes that hypertrophy which generally accompanies this state.

The passive cardiac aneurism, an enlargement accompanied with an attenuation of the parietes, occurs but seldom, and then almost always as an effect consequent on phthisis.

The dissection of bodies of such individuals as have died of consumption has frequently presented a true atrophy of the heart: its origin has appeared explainable by the circumstance, that in consequence of the imperfect hæmatosis which must take place after the disorganisation of the lungs, the heart also partakes of the general decrease.

The symptoms by which complications of the heart commonly manifest themselves during life are known, and in general easy to be perceived. A widely-extended and reinforced pulsation of the heart is not, however, always a proof of its dilatation; it is sometimes caused solely by a considerable condensation of the surrounding pulmonary texture; hard bodies, as we know, communicating the sound better than soft ones. A remarkable instance of this kind recently came under my notice in a consumptive youth of twenty-one years, whose right chest, down to two fingers below the nipple, gave an extremely dull and dead sound, whilst the entire left possessed an extraordinary clearness; nor did the sound of the heart, audible on percussion, exceed the ordinary limits: but, at the same time, the pulsation of the heart almost in the whole right half of the chest was so exceedingly strong, that I suspected the existence of a considerable aneurism of the heart. However, on examining the left chest by auscultation, I found the pulsations of the heart not only limited to their ordinary space, but also regular as to rhythmus and force.

The dilatation of the right cavities of the heart, accompanied with an hypertrophy of their parietes, when it has been brought on before the development of the tubercles, or independently of them, is distinct from a similar state which is the consequence of phthisis. It differs not only by the greater intensity of the symptoms, but also by their diversity as to quality: for, in the former, we always find a considerable oppression and difficulty of respiration which increases on the smallest movements, a great discomfort when lying on the back, a swollen face, puffy bluish lips, frequent bleeding at the nose, and a cough accompanied by spitting of blood, the spitting differing from the real phthisical hæmoptysis by a much darker colour of the blood, which is not diluted by phlegm, but ejected in a pure and unmixed state: the jugular veins, besides, are strongly filled and fluctuating, and we not unfrequently distinguish a bellows-sound and purring tremor, which may sometimes be produced by the reflux of the blood through the then much widened orifices of the heart. The diagnosis of this cardiac disorder, which is primary to phthisis and accompanies it, is facilitated in young consumptive persons by their peculiar habit; for they distinguish themselves by a retarded youth and a defective development, and sometimes by a moderate curvation of the vertebral column; they are childish, irritable, inclined to shed tears; their colour is of a venous cast (in opposition to the arterial, commonly called the scrofulous colour), and their eyelashes are long and black.

These two groups of cardiac affections, which differ with regard to their origin, have also a very dissimilar influence on the prognosis of pulmonary consumption. That dilatation of the heart which is an effect of phthisis, opposes no active impediment to the treatment of the pulmonary affection; and, after the removal of the latter by means of the oily frictions, it disappears gradually, and chiefly by itself, or it yields to a proper posterior cure, and to the cautious use of ferruginous remedies. The other primary form of cardiac dilatation, on the contrary, renders the cure of the phthisis very difficult, or, perhaps, impossible, because the continual excessive determination of blood towards the lungs limits the efficacy of the oleum animale fœtidum, impedes a free passage through the air-cells, hinders their dilatation; favours, on the contrary, the formation of new and the development of existing tubercles, and destroys, by repeated coughing and spitting of blood, the advantages which had, perhaps, been gained.

Even supposing a more or less complete cure of the consumption of the lungs had been obtained in spite of all these unfavourable circumstances, yet the patient can scarcely rejoice at this partial success, because there still remains the other primary disease, the phthisis cardiaca, which sooner or later must prove fatal.

In order to illustrate how useful the application of this oil, nevertheless, may prove in such most unfortunate complications, by, at least, retarding the rapid progress of the pulmonary disease, I shall now state the following

Case.—A boy, of sixteen years, whose parents, it was said, had both died of consumption, had been for six months affected with a difficulty of respiration, and particularly with a frequent cough and expectora-

tion. On my examining him for the first time he presented all the pathognomic symptoms of a consumption of the lungs, much advanced in its second stage. The left subclavian region was the principal seat of tubercular degeneracy, and a distinct pectoriloquy could be perceived there: but also in the upper part of the right chest the sound on percussion was sensibly damped, and the respiratory murmur much diminished. There existed, at the same time, manifest signs of a dilatation of the right heart: the dead and dull sound in the region of the heart extended from the fourth to the seventh rib. The extent of the pulsation of the heart corresponded with that of the dull sound, and was even perceivable till below the sternum. Its shock also seemed to be increased, and the pulsations were never less than one hundred in the minute. The two sounds of the heart, which rapidly succeeded and almost joined each other, were in no proportion to the frequency of the contractions; there existed at the same time, highly-filled jugular veins; a considerable difficulty of respiration; palpitations whenever he made any effort; a pale swollen face, and the entire habit which has before been described, and is so very characteristic of this cardiac affection. The use of the animal fœtid oil being resorted to, there appeared a considerable amelioration in the first six weeks; the frequency and violence of the cough, as well as the quantity of expectorated matter, had diminished; the patient gradually recovered his strength, and even increased in flesh; he was enabled to take walks, and appetite returned. But the progress went no further; and although the treatment was continued for six other weeks, no greater advance was made towards convalescence. On a later and accurate repetition of my examination, I scarcely found a change in the phenomena of the heart. The quickness of the pulse had little abated; its irregular accelerations only had almost entirely subsided. As to the chest, on the contrary, the resonance of its right side had become quite regular, except within a small space between the second rib and the collar-bone; some favourable change of sound had also taken place in the left chest, as far as I can recollect its prior state. But the improvement of the respiratory murmur was most remarkable; it was now distinctly audible even at places where before it was entirely wanting, and which still gave a rather faint sound, i.e., immediately upon the tuberculous excavations; there, of course, this murmur was weaker than at the deeper places of the thorax. Between the first and the second rib of the left side, I heard, instead of the former rattling, a loud and clear liquid splashing, which change I am unable to explain. The pectoriloquy at this particular place had rather increased than diminished.

Although these results of a treatment of so long a duration were small in comparison with those which might be wished, yet were hardly to be expected, still, under such unfavourable circumstances, they must be taken in account, and considered as extraordinary ones. The influence of the animal fœtid oil had not only succeeded in arresting the progress of the disease during so long a period, but even in procuring a considerable amelioration. Six months afterwards I learnt the death of this patient, whom I had no longer seen during that time.

CHAPTER III.

OF THE REMEDIAL OPERATION OF THE OLEUM ANIMALE FŒTIDUM IN CASES OF CONSUMPTION.

The easiest and most certain way to arrive at a correct judgment of the therapeutical operations of this remedy, as well as of the phenomena which it produces during the treatment of pulmonary phthisis, is to bring in juxta-position the cases in which, from the earliest times, cures have been observed as having been either designedly performed by art, or accidentally caused by the aid of nature alone; to compare with each other the respective efficient causes of these cures; and lastly, to consider the vital phenomena and organic changes which accompanied and followed these prosperous terminations. From this we shall be enabled to deduce a theory on the effects which are common to the different influencing means of these various cures, and on the general healing process in consumptive cases; and it will be highly probable that the new treatment, which is the subject of this treatise, will be found to differ solely by the use of a new remedy, namely, the animal fœtid oil, and to produce its salutary effects in plain conformity to the very same theory.

There is scarcely a remedy in our materia medica that has been left untried in this frequent and destructive malady. But from the most ancient times salutary effects have been expected almost exclusively from the application of internal medicines: for it had become customary to consider phthisis pulmonalis as a general, not local, disorder, as also to ascribe to each remedy some specific effect not merely on single organs and parts, but also on distinct forms of this distemper. But, in one respect as well as the other, the reverse is the fact; for, as to the former of these opinions, it is now-a-days evident that phthisis pulmonalis is no general disease; it is a local one, and solely confined to the lungs. Such alone could be called a general disease, of which the pulmonary tubercles are the consequences and remains.

And, as to the second opinion, medical science does not any longer acknowledge the existence of specific remedies under the above-mentioned acceptation of the word

All medicinal substances, when incorporating into the fluid and solid masses, can have an effect on a particular part which they do not touch, but when they change the state of these masses and their powers and qualities in general, and, consequently, also the particular ones, each in their kind more or less, such internal remedies, therefore, as were hitherto employed against consumption as against a mistaken general disease, or under the misconception of their specific effects on single and untouched parts, could, at the utmost, only produce general effects; and by their means we may indeed act (and sometimes favourably so) on the cause of causes of tubercular consumption, i.e., on the pervading tuberculous dyscrasia, but never directly on the pulmonary disease itself, the local effect of that general cause. Such trials, therefore, must necessarily be as insufficient for the cure of consumption, as an experiment would be, attempting the cure of a chilblain by internal medicines. Several evils which have a merely local appearance can, it is true, be cured by a general treatment; for instance, an ulcus syphiliticum by the internal use of mercury. But in these cases the effect of the remedy does not consist in the cure of the local disorder, but in the removal of its general cause; and the local evil will, after the removal of this cause, naturally heal by itself, in case it has not yet gained a proper topical foundation, on which it can maintain itself in its own substantial and independent character. Now this is precisely what takes place not only in various external inveterate ulcers of an idiopathic as well as constitutional nature, but likewise more particularly in tubercles and ulcerations of the lungs, where this own independent existence of the disease is partly founded on the organisation and functions of the respiratory organs, and partly on the peculiarity of the primary and generating disorder.

Hence it arises that in defiance of the many endeavours to discover a universal medicine against the genuine pulmonary consumption, and in spite of the multifarious recommendations of such pretended specifics, these have hitherto proved wholly inefficacious as remedies against an already-developed phthisis; some of them possess only a very limited applicability, which operates so far as their efficacy refers either to the primary cause and origin of the tubercles, or to some few phenomena and effects of the phthisis. In the use of the former consists the prophylactic; in that of the latter, the palliative treatment.

The treatment of the local affection, and, consequently, of the disease itself, by means of a direct and topical action on it, has, on the contrary, at all times been much neglected, and consisted at the utmost in the observation of some dietetical rules. The ignorance as to the real essence of the phthisis was, as has been before said, the cause of this neglect in earlier times. Latterly, the impossibility of arriving at the diseased parts, and consequently the great difficulty of applying the remedies directly on these places, have diverted the physicians from filling up this chasm which, though so fully indicated, has been scarcely attended to, and only been historically mentioned. It is particularly owing to this omission that a systematic classification and fixation of the indications according to which pulmonary phthisis is to be treated, remains so difficult and incomplete.

Certain physicians of recent times, nevertheless, were strongly impressed with the necessity of the local treatment of consumption; but their voice was not listened to, possibly because their imperfect intuition did not rise to a distinct conviction founded on clear motives, or because the remedies they recommended proved insufficient. Occasional experiments, indeed, were crowned by success, the cure having been effected, if not in the mode which they had in view, yet in another which happened incidentally to be connected with their own, and remained undiscovered. Still the repetition of these experiments, as they were established on a defective principle, necessarily failed, and the remedy was soon forgotten. Thus it came that physicians continued satisfied with the palliative treatment, which, it must be confessed, does not attain its professed, though limited, object of alleviation.

ON THE

SUBJECTIVITY OF DISEASE.

By W. Hughes Willshire, M.D., M.B.S.,
Physician to the Fore-street Dispensary, &c.

IS DISEASE MERELY THE NEGATION OF HEALTH, OR IS IT SOMETHING POSITIVE?

It appears to us that the more recent discoveries of the German physiologists, with respect to the phenomena of what may be termed the *theory of cellular evolution*, and which involves the fact, that the totality of every organism is derivable from an assemblage of elementary cellules possessed of individual lives, are confirmatory of the truth of the subjective essence of disease, and that, therefore, a cursory view of its leading principles may not be out of place, before drawing a conclusion which they seem essentially to involve.

Schleiden first demonstrated the fact, that the growth and increase of plants ensue by an evolution of cellules within cellules, and that this procreation of new cellules takes place by virtue of a power inherent in each, and which, therefore, may be said to possess a distinct individuality. This principle of evolution, demonstrated by Schleiden, was

shown by Schwann to hold good with regard to the animal kingdom, and, therefore, that there is a coincidence in the laws of their general growth. The law of this evolution may be thus expressed:—The cellule once formed, increases by its own individual inherent power, but is influenced by that of the whole organism, the effect to which the latter tends directing the progressive and determinate evolution of the former; and the coercion of this law is so fundamental, that it operates within the *primordial* cell as well as with respect to the evolving ones.

From further investigations of Schwann, Stark, and many others, it is also deducible that formation, nourishment, growth, secretion, and excretion, in fact, the whole process of the self-maintenance of the organism, takes place at every portion of it by virtue of the freely active *productivity* (plasticity), and the power of chemical metamorphosis with which these individualised cellules are endowed. The self-maintaining power of these cellules, their nourishment and growth, has been seen to exist in the primitive cells of the ovum long before their possession of a vascular or circulatory system could be demonstrated; all portions of the ovum in the earlier stages of the development of the embryo increasing by means of the individual cells, each cellule containing within it a source *by* which and *from* which the younger and new cells are generated. In more advanced stages of animal life the blood, and more especially what have been denominated by some German writers the *blood-cells*, become mediate agents in the nutritive action, conveying the general nutritious material to the elementary cells, which, by virtue of their individual and collective powers, convert it into their own nature. The blood, therefore, only plays the part of a mediate agent in the nourishment of all organic structures, whilst the cells themselves, by virtue of their plastic and metamorphosing powers, undertake the assimilation of the nutritious material: thus are seen several portions of the body nourishing and maintaining themselves, though out of the direct and immediate influence of the circulation, or, at least, contact of vessels, and thus coinciding with the plastic processes observed to take place in vegetable structures. The process of self-maintenance taking place by means of the minute organisms (the cells), has for its conditions a continual self-procreation at all portions of the body, and which, in other words, is a constant individualising (that is, the formation of cells, possessing each inherent life of its own); but running parallel with this power and action is seen a resolution of the individual life into the general influence exercised by the totality or universal organism, and by which an ever restless change, or circle of organic activity, is kept up and preserved until death comes on.

Now, if all tissues, organs, and systems are composed of simple individualised cells, it will follow that the process of disease falls within the sphere of these minute organisms, and must be viewed as an anomalous form of cellular life or activity, which can no longer act in harmony with, nor is influenced by, the universal tendency before alluded to. That the essence of disease has been often regarded as an altered fundamental power of life, possessed of an anomalous formative activity, is true; but from our want of knowledge of the essential nature of this formative activity, and by our continually changing and erroneous notions as to whether the nervous system or the vascular apparatus were primarily or chiefly engaged in it, and from our not allowing that disease may not be the product of either of these systems, but happens through the independent, plastic, and metamorphosing powers of the cellules, disease has only been viewed in connection with a general systematic change, and much less frequently with the special change of a local form of life, or what may be termed with Sobernheim the *topical individuality*.

That in inflammatory affections an altered or diseasedly-excited formative action is present, is evident from their production of morbid growths, formation of membranes, hypertrophies, and so on; and it is true, that this has been acknowledged, but that these *after-productions* grow, and are evolved according to laws of formation, regulating the whole organism itself in its primordial development; namely, out of the cells, and their power of evolution: that these productions are just as much *after-lives*, as what we denominate healthy structures, proceeding from and produced by a special, ultimate, and objective mode of life in the cells, has not been so acknowledged until very lately, indeed, by a few German physiologists. Allowing such to be the origin of the *evolution* of diseased life, it is evident that two strictly subjective conditions may be existing in an organised body at one and the same time, namely, health and disease; and, therefore, the latter is not a mere *negation*, but something *positive*. Diseased life develops itself, like healthy life, out of a nucleus, and has just as much an essence or substratum at the bottom as the other has, and is no more the negation of health than evil is the negation of good. Though diseased life organises and connects itself with the general organism in manifold and very manifest ways, as, for instance, in the production of tubercle, carcinoma, or melanosis; and if its presence is not thus immediately accompanied by such evident plastic operations, it is yet apparent to us in its existence by the want of harmony observed in the general life of the whole body, and which at the bottom is just as much brought about by a difference in the objective powers acting in the individualised

portions of the economy (the cells) as the more palpable forms of diseased life were, since all power and action ultimately arise there. In such cases where we see these two objective forms of life carried on in a *living* body, there is only a partial isolation of the life of the individual cells to a greater or less extent from each other (the diseased from the healthy), and by means of which a discordancy of the phenomena we imply to exist in a healthy body is observed to take place. Should this isolation be extensive, should the diseased life overpower and negative the healthy, by giving the universal organism a new tendency, then death supervenes, and the whole individualised portions of the body (the cells) take upon themselves a new objective mode of life, which is that of death. That the powers upholding or regulating the two forms of life allow of a difference, in some respects, is evident; for, whilst the organism is existing in the condition of healthy life, partial isolation of the life of the cellules into that of health and disease may take place, and yet again be brought into a harmonising condition as respects an universal healthy life of the organised body; but whilst the body is existing under a condition of diseased life (the moment after death), no alteration can take place as regards the *totality of the same system* in its repossession of healthy life, no new isolation in this respect of the lives of the cells into healthy and diseased life, they can only take on the former in connection with the formation of new and distinct organisms, forming creatures of life lower down in the scale of creation.

We would, therefore, agree with Stark and Sobernheim, that disease is not the *negation* of health, but is something *positive;* and that in the theory of cellular evolution is to be sought the origin of all pathological formations; that all forms of exudation, membranose and pseudo-membranose structures, tubercle, carcinoma, hydatids, and moles, &c., are ultimately dependent upon a peculiar objective form of cellular life, as well as obliterations of vessels, hypertrophies, and hepatisations, &c., and we would now inquire into the true nature of the subjectivity of disease.

In the present condition of our knowledge with regard to the primordial development of disease, it must be confessed that any settled view with relation to its subjective essence is out of the question; all that we appear to be sure of is its actual subjectivity: taking everything into consideration, however, the following appears to us as a warrantable deduction from already accumulated and well-known facts. What we term diseased life may be in its essence only a lower form of animal or vegetable life to that body in which it is seen to occur, and, therefore, possessing the same objective essence as healthy life does with respect to the whole organism of the *new* creation, but not with respect to that of the more highly organised body in which it occurs; and it is this parasitical possession of a portion or whole of the totality of one body by another less highly developed, and of a lower form of life, which constitutes evident disease, to us a less evident disease is seen, by the approach of a tendency to allow and admit of such parasitical possession. It is true that the *existence* of a lower form of life in every disease is not apparent to us, but in others it is; and it may be thought that whilst the body will admit of the *full development* of a new parasitical creation of certain kinds, or under certain conditions, and yet live, so in others it will not, the mere tendency or preparation or progress to such full development, being alone necessary to break up the harmony of the system, and bring about death: disease may, therefore, be expressed by a form of Okens—the pathology of man is the physiology of animals; or else by another—the pathology of one being is the physiology of another. A few illustrations may be allowed us: the actual development of lower forms of life to the body in which they occur are phenomena of certain diseased conditions of the higher organised, as, in fact, are its disease; and the existence of hydatids, as acephalocystis, cysticercus, cœnurus, echinococcus, &c., prove it: a low form of life exists in the eruption of scabies; a still lower form of life, as shown by Donné, in carcinomatous structure. Schönlein has proved the existence of a low form of life in the pustules of porrigo lupinosa; and Langenbeck the presence of it also in small-pox and typhus. In diseased lower animals the same new creation has been seen. Laurent discovered a destructive form of life in an embryo; Hanover new creations in reptiles; and Ehrenberg in fish; and the researches of Bassi and Adouin upon the silkworm, abundantly illustrate circumstances as similar.

This question may, however, divide itself into two parts; namely, Is this low form of life, the return to which, or the tendency to admit of it, constitutes the subjective essence of disease, to be regarded as produced by a retrocession, as it were, of the higher life into a lower form, and thus produced by a sort of *descending metamorphosis;* or it is to be viewed as a parasitical individuality, a form of life superadded to the other form of life existing in the organism?

Buckingham-street, Adelphi,

Oct. 1, 1841.

CASE OF

FUNGUS HÆMATODES

CURED PRINCIPALLY BY LIGATURE.

By Charles Clay, Esq., Surgeon, Manchester, Lecturer on Medical Jurisprudence, &c.

Hugh Carleton, ætat. 60, of China-lane, Manchester, applied to me for relief under the following circumstances:—Nearly two years ago he was engaged in fixing some iron piping five or six feet from the ground, when the end fell down and struck the nipple of his right breast a severe blow. As the painful sensation was soon over, he thought little of it; but in a few days a swelling, the size of an egg, appeared, with considerable pain. He applied to a surgeon, who plunged a lancet in it, and a quantity of almost colourless liquid was evacuated; contrary to expectation, the tumour did not subside, but from that time to the period of his consulting me, it rapidly increased, and when I first saw it, its probable weight was near three pounds, highly vascular over its whole surface, extremely painful, and attended with profuse hæmorrhage on the slightest exertion. In consequence of the latter, the patient was reduced to the last stage of debility. The circumference of the tumour round its base was about twelve inches, and at its apex about fifteen, and stood out from the chest at least eight inches. The whole of the upper surface of the mass appeared as full of vessels as a placenta. The constant danger from hæmorrhage rendered it imperatively necessary that something should be done, whilst its situation and vascularity condemned any attempt at extirpation by the knife; for the present I simply applied common dressings of lint, wet with astringent lotion, and advised the patient to be very quiet, being determined to watch the case closely, and act as future circumstances rendered necessary. I was not kept long in suspense; I was hastily called to visit him on the 12th of July last, and found life all but extinct from hæmorrhage. I instantly passed a strong ligature round the neck at the base of the tumour, and drew it very tight. I had the greatest possible difficulty in restoring my patient to animation, and for three days he lay in an almost hopeless state, from extreme exhaustion, and the excruciating pain of the ligature. On the 16th, I passed a second ligature round the base, and afterwards cut away the first. This was attended with great pain, and required powerful opiates to obtain some little mitigation of suffering. My patient now had frequent fits of delirium; his appetite entirely left him, and he appeared rapidly sinking. I felt doubtful as to proceeding, but as the case offered no further prospects on desisting than proceeding, I determined to go on with the ligatures, particularly as the patient in his lucid intervals desired it. I therefore renewed the ligature every third day, and found it was rapidly cutting through the base, leaving a healthy surface behind. From the first application of ligature no hæmorrhage had occurred.

July 27. The parts now uncut by the ligature appeared less vascular than those cut through, and in great apprehension of my patient sinking from exhaustion, and the severe pain attendant on every new ligature, I concluded to cut it entirely away by the scalpel, which the poor man willingly consented to. A healthy-looking sore now presented itself, which, from the excessive debility prevailing, was extremely slow in healing.

I have now, Sept. 20, dressed it for the last time. No appearance of fungus, and the man is quite well, excepting debility, which is daily giving way to a generous diet and tonic medicines, which is considerably more than I ever anticipated from the severe nature of the case, its situation, and the advanced age of the individual; but it serves to show what perseverance and determination will effect. In this case, the conduct of the patient was beyond all praise; there are few, indeed, who would have suffered so many renewals of agonising pain.

It is more than probable that many cases of this formidable disease might be treated successfully by ligature, as it is my opinion, in many circumscribed cases, the diseased mass is not so deeply connected with the system as to be pronounced hopeless. Few cases bore a worse or more unpromising appearance than the one here quoted, and yet it has been obliterated by local means. In such cases, where frequent hæmorrhage is a prominent feature, I should not hesitate to apply the ligature for a few days; then, when the parts included were sufficiently deadened, I would cut through them. My first intention, in the case here given, was to use a silver wire ligature, with a running noose, that could be tightened daily without removing; but I soon abandoned that idea, because I found it was a ligature for pressure more than cutting that is required for the first few days: when the mass included is sufficiently dead, a silver wire might be used as cutting more freely, but at this stage, to save the patient's suffering, I should prefer the knife. The ligature I made use of was a piece of strong greased tape.

PUERPERAL CONVULSIONS.

To the Editor of The Lancet.

Sir,—The following concise detail of a case in midwifery, accompanied by most violent convulsions, is at your service for publication. I have retired from the general practice of my profession in consequence of the impaired state of my health.

I was upon a visit to my son, Dr. George Morley, of Swinderby, near Lincoln, this summer, when, on the 14th of July, I was sent for by Mr. Johnstone, a respectable surgeon of Bassingham, in consultation upon an extraordinary case of midwifery. I had not been in the patient's room two minutes before she had a return of a convulsive fit, and of the most alarming character, accompanied by gnashing of the teeth, foaming at the mouth, countenance and hands black, and requiring several persons to keep her in bed. We immediately held a consultation, and decided that the first step to be taken should be bleeding, which having been accomplished, we waited for the result, and in less than ten minutes our patient had a return fit, and though truly alarming, it was not so violent as the preceding one. Under these circumstances we thought it prudent not to proceed to delivery by artificial means, until we had given further time to see what nature would do. My arrival at the patient's house was between eight and nine o'clock in the morning, and ten o'clock having arrived, the convulsions still continuing, and there being no prospect of delivery from the efforts of nature, we concluded that the most judicious mode of proceeding, with a view of saving both the life of the mother and child, would be to deliver by artificial means; and for this purpose Mr. Johnstone placed the case solely into my hands, and, I think, much to his credit, I being the much older practitioner. I felt that a great responsibility was delegated to me—that of the life of two individuals, mother and child. I applied the vectis soon after ten o'clock in the morning, and in three-quarters of an hour I effected the safe delivery of a living child. Before this was accomplished, I informed the attendants and relatives, that in all probability the convulsions would continue after delivery, which was the case, but in a greatly mitigated degree. Our patient was totally insensible at the time of delivery, and continued so several days; but by the subsequent judicious treatment and great attention of Mr. Johnstone, both mother and child, at the end of the month, were as well as if the case had been of an ordinary character.

The treatment consisted mainly of an occasional use of the catheter, enemas, anodynes, with cold applications and blisters to the head; for we were of an opinion that the proximate cause of the convulsions was an undue pressure upon the brain.

Upon my return to Hull, I referred to Dr. Smellie's publication, the edition of the year 1766, a popular work on midwifery; and I found four cases detailed by him attended by convulsions, one of which proved fatal, the doctor imputing it to procrastination, occasioned by the folly of interference on the part of the relatives. In my younger days I had read Dr. Smellie's work; but his mode of practice did not recur to me in the present case, and had it, I am pretty sure I should not have pursued it, for I find he preferred turning and delivering by the feet, even in *natural presentation*. Cases may occur which would sanction such a line of proceeding; and one has occurred to me during my residence in Hull, the placenta having been placed over the os uteri, with a strong morbid adhesion, attended by profuse flooding. In this case I effected the delivery of a living child by turning and delivering by the feet, and both mother and child did well. But in the case of convulsions (and it is fortunate they are of so rare an occurrence, for notwithstanding I practised in midwifery between thirty and forty years, and that extensively, at Wellingore, in the county of Lincoln, I never met with a case with the exception of the one just recited), being of an opinion that the proximate cause is an undue pressure upon the brain, it appears to me to be more reasonable that such artificial means should be used as are least likely to increase that pressure, and in my opinion either the forceps or the vectis, when carefully and judiciously used, will effect this; I mean only when the presentation is natural. Before I conclude, I will mention that, in applying the vectis in this extraordinary case, I made no extracting force whatever, only when the *natural pain returned*; during the convulsions I ceased altogether, and as both the mother and child did so well under this treatment, the fact will bear the repetition which I have just given, there was no violation of nature's rules, only an aiding of them. I remain, Sir, your obedient and humble servant,

JOSEPH MORLEY, M.R.C.S.,
Late of Wellingore, Lincolnshire.
Hull, Oct. 14, 1841.

ULCERATION OF THE STOMACH, FATAL BY PERFORATION.

To the Editor of THE LANCET.

SIR,—The following case, as illustrative of a very insidious and fatal disease, may perchance merit a place in the pages of your Journal. I have the honour to be, Sir, your obedient servant,

STEPHEN H. WARD.

7, Wellclose-square,
Oct. 18, 1841.

W. H., a carman, forty years of age, of a lymphatic temperament, having previously enjoyed moderately good health, was seized a little before one on the morning of Tuesday, October 12th, with excruciating pain in the abdomen, more especially referrible to the epigastric region, attended, however, by so violent a spasm of the abdominal muscles, as in a great measure to mask the more immediate seat of pain. These symptoms were accompanied by incessant vomiting, consti-

pation of the bowels, a small, frequent, rather wiry pulse, and general coldness of the surface. His countenance was expressive of great distress, and he could scarcely attend or reply to questions put to him.

When first seen, the state of the pulse, &c., seemed to contra-indicate the use of the lancet, but a full opiate was administered with a view of relieving the spasm. After a lapse of two hours, reaction had, to a certain extent, taken place, the surface was warmer, and the pulse had increased in fulness. As the spasm and pain continued without the least abatement, I abstracted sixteen ounces of blood, and repeated the opiate. The bleeding for the time produced slight relaxation of the abdominal muscles, with alleviation of the pain. After a short period, however, the symptoms returned in their original intensity; an injection of gruel and opium was administered, but nothing seemed to afford the slightest relief. He continued in the greatest agony during the whole day, and expired between eleven and twelve on the Tuesday night, about twenty-three hours from the commencement of the attack.

Inspection.—The abdominal cavity contained about a quart of fluid, of the colour and consistence of thin pea-soup. The peritoneum exhibited marks of recent inflammation, and the intestines were slightly adherent to each other and to the abdominal parietes. The stomach was collapsed, and exhibited in its greater curvature, towards the pylorus, an ulcer of irregular shape, with smooth, thickened edges. This had entirely destroyed the internal coats, leaving only the serous investment, which had given way, forming an aperture through which the contents of the stomach had escaped, and which was sufficiently large to admit of the finger being introduced through it. Extending from this ulcer along a considerable portion of the great curvature, the mucous membrane presented extensive superficial ulceration. The small intestines contained only flatus. The liver, spleen, and thoracic viscera, were perfectly healthy.

Dr. Abercrombie in his classical and very excellent work on Diseases of the Stomach, insists much on the insidiousness of this affection, and on the very doubtful character of the concomitant symptoms. These, as he observes, although at times severe, are in many instances merely those of dyspepsia, so that the greatest difficulty is experienced in forming a correct diagnosis. Of the present case, as the man had had no medical advice for some years, the history is of necessity involved in great obscurity, although, as he was capable of pursuing his occupation up to the day preceding his death, he could not have been the subject of much acute suffering. From what I could learn from his associates, he had for some years been very irregular in his habits, and at times much addicted to intemperance. Of late, from the inconvenience experienced on taking food, he had suffered long intervals to elapse between his meals, and had had recourse to stimulants to supply the defect of proper nutriment.

During the fatal attack, the spasmodic and tense condition of the abdominal muscles, and, consequent I suppose upon this, the constipated state of the bowels, tended much to disguise the nature of the disease.

COLICA PICTONUM,

FROM THE MEDICINAL EMPLOYMENT OF ACETATE OF LEAD.

To the Editor of THE LANCET.

SIR,—Should the following case, in which colic was produced by the administration of lead during an attack of severe hæmoptysis, be deemed sufficiently interesting, you will, by its insertion in your valuable Journal, confer a great obligation upon your obedient servant,

JOSEPH B. GRAY.

Chelmsford, Oct. 14, 1841.

Esther Brown, ætat. 27, of regular habits, but of consumptive diathesis, was placed under my care September 24th, 1841. Four or five years since she had a slight attack of spitting of blood, but since that period has enjoyed tolerable health, with the exception of occasional attacks of cough, which have occurred at intervals during the last two years. Her present illness occurred on the night of the 23rd, or rather early in the morning of the 24th, when, without having experienced previous pain or uneasiness at the chest, she suddenly awoke with feelings as of impending suffocation, which were relieved by the expectoration of a considerable quantity of blood. She coughs slightly before the blood comes up, which produces a sensation of choking, at which she feels great terror; the blood is discharged in half-mouthfulls; respiration slightly accelerated; considerable dulness and crepitation on the lower part of the left side of the thorax; pulse quick, jerking, 110; bowels open. Anxious, if possible, to save her farther loss of blood, I contented myself with prescribing the application of cold to the chest, and a grain of the acetate of lead every three hours. To quench her thirst, she was recommended to sip frequently of cold vinegar and water.

26. The bleeding has returned, and she has during the night discharged at intervals nearly a pint of bright red blood; the pulse very quick and jerking; respiration quick, heaving, and attended with pain at the lower part of the left side of the chest; countenance very anxious. She was bled to a pint; the lead increased to two-grain doses.

Evening. The blood is brought up in less quantity; there is more cough; pain at the chest continues; bowels confined. An ounce of castor-oil directly; twelve leeches

to be applied to the chest; the acetate increased to three-grain doses; flannel dipped in heated turpentine to be applied to the chest.

27. Pain at the chest much relieved by the leeches; only an ounce and a half of dark coagulated blood expectorated to-day. To continue the remedies.

Oct. 2. Up to this date the case proceeded very favourably; the expectoration of blood gradually and progressively diminishing; the pain at the chest entirely gone. This morning she awoke with much pain and griping about the umbilicus; bowels confined; pain not increased by pressure. To have a mixture with ℥iss of Epsom salts in eight ounces of infusion of roses, and half a drachm of laudanum. Three tablespoonfuls every two hours. Warm fomentations to the abdomen.

3. The pain in the bowels still continues; they have not been moved; the hæmoptysis has entirely ceased. A tablespoonful of castor-oil every four hours.

4. The pain in the bowels still very violent, although several doses of Dover's powder have been administered during the night. She has not had a sufficient evacuation for three days. A turpentine enema to be administered directly; twelve leeches to the abdomen, followed by an opiate and turpentine epithem.

5. The pain is much relieved; bowels have been acted upon four times; there is no spitting of blood; she only complains of a little occasional griping, with flatulence. Nothing worthy of notice occurred in the subsequent management of the case, and on the 12th she was deemed sufficiently well to return to Deptford, the residence of her parents.

CURE OF LOCK-JAW BY AN ACID PREPARATION.

"Case of Lock-Jaw.—* * * Mr. Wakley said that the deceased lost his life through the injury accidentally sustained in the hand by the rusty nail," &c. * * * "To counteract such symptoms, the whole materia medica had been searched, and all kinds of means adopted, but in vain; all had failed." —*Morning Newspaper.*

To the Editor of The Lancet.

Sir,—Having read the above extract in the newspapers, I am induced to address you on the subject of lock-jaw, as I conceive it to be the duty, on the score of humanity, of any person who has been relieved from a complaint by the successful application of a drug, to state the case for the consideration of the faculty.

In the year 1807, having contracted a dysentery whilst serving in a vessel of war, on the Spanish Main, on our arrival at Port Royal, Jamaica, I was sent to the Naval Hospital. Very powerful medicine was administered, but through the neglect of the nurse in having the jaloucies (large Venetian blinds) open during a strong sea-breeze, I "caught cold," and the consequence was a lock-jaw. The attempts of the surgeons (all attached to the ships in harbour having attended) to relieve me from the distressing symptom, failed; but I was speedily cured by the application of a dark-coloured viscous liquid, resembling tar, administered by a black woman, with a feather; the head reclined backwards, so as to admit the liquid to pass between the interstices of the teeth. The taste, or flavour, of this liquid was powerfully *acid*, with a slight sweetness blended, and the relaxation of the muscles took place in a very short time afterwards. I do not now recollect how soon, but I believe within an hour. I was in dreadful torture, so exquisite as nearly to create swooning on every occasion of deglutition. The dysentery, although reduced, was not cured.

I went into the country for change of air, and there got entirely rid of my troublesome disease by the use of the astringent juice of the plantain sucker (*musa*).

After having sojourned there for a little time, I joined my ship, and unfortunately got wet, the consequence a sore throat and lock-jaw again! The black woman was immediately sent for, and upon her applying the same liquid as done in the former case, the same result followed; *after* which a small abscess burst in the throat, and I became perfectly restored. It was with extreme regret I found that no money offered would induce the woman (an African), from whom the drug was obtained, to part with the secret. Her grand-daughter (my washer-woman), who administered it, did not know what it was; and she refused all recompense, being exceedingly indignant when I tendered her a doubloon, preferring to do good without fee or reward.

The substance appeared to me to be the rob of the island sorrel, a plant very different in appearance to that of our fields, the *rumex acetosa*. Some of the sick officers, however, considered it to be "Barbadoes tar;" I am not aware of that petroleum possessing an acid flavour. It appeared to me that the effect of the relaxation of the joints of the jaw proceeded from the action of the powerful acid on the nerves, or the muscles. The water of the salivary glands spurted out most copiously the instant the liquid reached them. This action nearly deprived me of life. The question naturally arises, would not *any* strong vegetable (and perhaps mineral) *acid* produce the *same effect*? It is worthy of trial in the hospitals. How far it would have been efficacious, had the tetanus under which I laboured, proceeded from a *wound*, I am not competent to judge. At all events, I trust you will excuse the liberty, as

a stranger, I have taken in making this communication to you. I have the honour to be, Sir, your very humble servant,

J. Evans, Lieut. R.N.

Bedminster, Oct. 12, 1841.

CURVATURE OF THE ARM, WITHOUT FRACTURE OF THE BONES.

To the Editor of The Lancet.

Sir,—I scarcely know whether it is worth while taking up the valuable time of your readers further on the subject, already discussed lately by three other practitioners, I refer to the "cases of curvature of the ulna and radius without fracture, in children." The subjoined case, however, is highly interesting, as proving the possibility of curvature without fracture, to a very great extent; and also, that it is by no means confined to infants and very young children, but may occur even at the age of ten.

A, elbow.
B C, back of hand.

August 5th, 1841, a boy named Charles Hatheway, ætat. 10, was jumping over a post, when he fell upon his wrist and bent his forearm. The annexed diagram gives the exact curve of both radius and ulna, traced off (by placing the arm upon the paper) by myself immediately after the injury. The treatment consisted in making continued extension and counter-extension from the wrist and elbow for a few minutes, and then in bending the bones carefully and gradually over my knee, still keeping up the extension for ten or twelve minutes longer. When they were brought nearly straight, pressure between two splints completed the cure in about ten days. Not the slightest inflammation followed.

I may mention that I have had two other cases of the kind, in much younger children, all within the space of a month (therefore by no means uncommon), and all the children of the poorer class.

Your obedient servant,

J. B. Kimpton, Surgeon.

Old Brompton, Oct. 12, 1841.

BENT BONE.

To the Editor of The Lancet.

Sir,—In the course of thirty years' practice having had to treat several cases of bent bones, I will, with your permission, relate the particulars of one of them, in order to prove that, in my opinion, your very respectable correspondent, Dr. Mantell, has committed an error in designating such cases "partial fractures," and to show that the mode of cure recommended by him is injudicious, and would be, if adopted by other practitioners, very detrimental to themselves. All the cases I have witnessed occurred in children; and in each case the peculiarity of this kind of accident left no doubt on my mind that it arose from a state of mollities ossium, produced by evident deficiency in the deposit of phosphate and carbonate of lime in the bones, rendering them tough and flexible, instead of hard and unyielding as they are in their natural condition. I am confirmed in this opinion by the state of the skeleton of a monkey now in my possession. The whole of the bones are thickened, spongy, and light: the long bones are all much curved, and when recent were very flexible, having much less of the earthy deposit in their structure than they ought to possess. Even now, in the dry state, full seven years since the preparation was set up, the long bones retain a considerable degree of elasticity. The animal died of a kind of scrofulous disease, such as tropical animals are subject to in this country. To illustrate the foregoing remarks, the following notice of a case is introduced:—

In 1816, a child, of about seven years of age, living at Chew Stoke, in Somersetshire, fell from the top of a high gate and injured her arm: the exact manner of receiving the injury I cannot now relate; but when I first saw the little patient, both bones of the forearm were bent inwardly, nearly to a right angle, and retained firmly in that position, so that it required considerable force to bring them back into the normal state.

To remove the deformity, recourse was had, in the first place, to gradual extension and pressure between the hands applied on each side of the limb; but finding these means ineffectual, the arm of the child was laid in the prone position on a table, when graduated and very firm pressure with both hands was applied to the convex surface of the arm, until the concave surface touched the table. Owing, however, to the fibres of the bones having been much stretched on their convex sides, even this pressure did not cause the bones to remain in their natural position, but they were easily retained afterwards by the application of splints in the ordinary manner. There was very little subsequent inflammation, and the child soon recovered the use of the arm.

The result of this case, as well as that of

all the others which have fallen under my care, clearly proves to my mind that it is perfectly safe and judicious to restore the bones in these cases *at once* to their natural position. Dame Nature is an excellent doctress, and must be kept in good humour, but every medical man knows full well that many cases are too mighty for her *unaided* efforts. To have applied splints and a bandage alone in this case would have been almost useless; to have left the bones bent almost to a right angle, and merely waited for the curative powers of nature, would have been preposterous.

The parents and friends of the child, I should think, would in that case have stared at me with astonishment, and in all probability I should ever afterwards have been subjected to well-merited reproach; even the bones themselves might have *elbowed* and annoyed me in silent derision of mal praxis, whenever I came in contact with them.

DISLOCATION OR NOT OF THE WRIST-JOINT (?).

If, Sir, a single fact presented through the respectable medium of your widely-circulated Journal will have any weight in settling the controversy lately carried on between your reporter and other gentlemen, I beg to offer one.

On the 3rd of September ult. my son was called to Mrs. Baggott, about sixty years of age, of Cannon-street, in this city, who had fallen from the top to the bottom of the stone cellar stairs of her own house; the whole weight of her heavy body having been received by her hands, with the exception of her forehead, which only received a slight contusion, and was well in a few days. Both bones of the right forearm were fractured at their lower third; and the radius of the left arm was broken at its lower extremity near the junction of the cartilage. The ligaments of both wrists were much strained and otherwise injured, but *neither of the wrists were dislocated*. The bones have knit, and the patient is fast recovering from the effects of the accident. I am, Sir, your obedient servant,

JAMES PROWSE.

Bristol, Oct. 6, 1841.

ST. MARYLEBONE INFIRMARY.

To the Editor of THE LANCET.

SIR,—Having been one of the first pupils that entered to the medical practice of the Marylebone Infirmary since its recognition as an hospital by the Apothecaries' Company, I feel in duty bound, as well as from other reasons, to reply to the letter of your correspondent in last week's LANCET, signed "A former Pupil;" in doing so, I will not say anything about the writer of that letter having *ever* been a pupil of the infirmary, further than that I am convinced there is no pupil that paid attention to the instruction given in that establishment, but will in after-time, when in practice for himself, have reason to look back with satisfaction and pleasure on the hours he spent in it. As for my part, I would consider myself guilty of ingratitude, in return for the pains taken by the honorary medical officers in giving instruction, if I did not contradict the statement of your correspondent, as far as relates to the opportunities which offer, and the instruction given in the practice of physic and morbid anatomy, which, if not superior, are second to no other in London.

At the infirmary there are admitted only a limited number of pupils, which is a great advantage to those who enter, as all may hear the remarks made by the physicians;—so different from other hospitals, where, in the scramble to get a good place, three-fourths of what is said is lost even to those close to the bed.

The statement of your correspondent, that the pupils are hurried through the wards, is altogether incorrect, as sufficient time is given to take notes; and remarks, to a small extent, are made at the bedside of each patient by the physicians. When I entered as pupil to the medical practice in 1838, the physicians then were Drs. Clendinning and Macreight; since that time a change has taken place in the medical staff, and the physicians who now give instruction are Drs. Clendinning and Mayo: of the former, I cannot speak too highly; of Dr. Mayo, who was but recently elected, I have not had the same opportunity of judging, as my attendance during the last six or eight months has not been very regular; but the few times I have been with him in the wards and dead-house, he was at all times willing and ready to give me any instruction or information.

I cannot conclude this without stating, that from Mr. Phillips, who has delivered more than one course of lectures free to all the pupils, I have, in common with the other pupils, received on various occasions much instruction.

In what I have here stated, I am certain I shall be joined by *all* the pupils of the infirmary, for I question very much whether your correspondent was a pupil. It sometimes happens that men, though qualified by the seal of some corporate body to enter into practice, may require instruction, though the proper days of their pupilage has passed: such may be the case with your correspondent. Whoever he is, he should not make a charge, such as he has made, without giving his name. To him I would say in the words of the poet—

"'Tis not enough, taste, learning, judgment join,
In all you state let truth and candour shine."

I am, Sir, yours obediently,

WILLIAM O'CONNOR,
A Pupil of the Infirmary for the last Three Years.

Wednesday, Oct. 13, 1841.

THE LANCET.

London, Saturday, October 23, 1841.

When the mind for a length of time has assumed a certain position to be *the right one*, it is not very quickly dislodged, whatever may be the batteries which are brought to bear against its maintenance. Say that the premises are unsound; declare that the point has been held by false reasoning—by the adoption of false "facts,"—what is the reply? That "the *right* has continued and been acknowledged for *twenty years*." But assuredly it ought to be seen and admitted that a privilege may have been enjoyed for even a much longer term, only because the right upon which the privilege is supposed to rest had *not been questioned*. The CHEMISTS and DRUGGISTS have been *assuming*, since 1815, that they enjoy the right *at law* to practise as APOTHECARIES in *their own shops;* and until lately the right on which that assumption was based, had continued undisputed in our *courts of law*. It is fortunate for the interests of the public that the question has remained for so many years in such a state of abeyance. The proceedings in which the Apothecaries' Company have been engaged, regarding physicians and surgeons who were practising as *apothecaries* in England and Wales, *without* the licence of the Company, have operated with prodigious force in determining the late decision in the Court of Queen's Bench. We again admit, and in the most unqualified sense, that when the Apothecaries' Bill of 1815 was enacted, it was not intended by the Legislature to interfere *in any manner* with the *trade* or *occupation* of the retail druggists of this country. We make this admission with the utmost frankness,—without the slightest reservation; and no doubt exists in our mind as to what would have been the result of a prosecution by the Company, directed against a druggist, for practising as an apothecary in his own shop, or even in visiting patients away from his own place of abode. In such case it is our firm and fixed belief that *the Company* would have been beaten, and that the prosecuted party would have triumphed. That the legal advisers of the Company entertained a similar opinion at that *early* age of the statute 55th GEO. III. is equally unquestionable. At *that* period the points mooted in a prosecution against a supposed offender of the law would have consisted simply of—the *provisions of the statute*—the *trade* of chemist-and-druggist, as carried on under the right secured to him at common law—and the then *known* and *recently-declared* object of *the Legislature* in the enactment of the new statute.

But, now, another ingredient must be added to the account,—in fact, the most weighty of the whole,—namely, that which has been formed by the decisions of the courts of law. *Precedent* operates upon the mind of an English judge with all the force and authority of a statute. Precedent is *law*. The Judges, therefore, in the practice of *law-making*, beat the Parliament quite out of the field. Far be it from us to contend that the decisions of the courts of law, and the provisions of the statute of 1815, have been antagonised in the prosecutions which have been instituted by the Apothecaries' Company. But it cannot be denied that the law as it was laid down by the Judges, and the *intentions* of the Legislature in passing the 55th GEO. III., were, in all the *early* prosecutions, placed in opposition,—in direct and distinct opposition. Still, it must be confessed that the Judges were right, and that Parliament was wrong. The Judges were required to administer the law as it was found expressed and laid down in the statute. If members of Parliament did not give effect to their *intentions* by making the law adequate to execute their intentions, the fault was their own, and not that of those functionaries who were bound faithfully to administer the law.

We say, then, that an authority of *equal, if not of greater, weight with the provisions of*

the statute, has been added to the law of 1815 by the decisions of the judges. What have those decisions been? Why, that physicians and surgeons belonging to our Universities and Colleges, men of the highest education and medical attainments, have no right to practise as apothecaries, in England and Wales, unless they have obtained the licence of the Apothecaries' Company. The judges have also decided that such physicians and surgeons, if they *do* violate the law by practising as apothecaries without being possessed of the regular legal authority, are liable to all the penalties of the Act of 1815; and, besides, that they shall not be entitled to recover at law the amount of any debt that may be contracted with them in discharging the duties of apothecaries.

Such had been the decisions of the judges long antecedent to the time when the motion for a new trial was made, in the Court of Queen's Bench, in the case of the APOTHECARIES' COMPANY *versus* GREENOUGH. After such decisions it was quite natural, when the question came to be mooted, whether a *chemist-and-druggist* should be entitled to practise as an apothecary, without a licence from the Company, that the judges should examine with extreme deliberation and marked scrutiny that clause of the Apothecaries' Act which the druggists held to exempt them wholly and completely from the operation of the Act of 1815. The judges naturally said, "How can we, after having "decided that *physicians* and *surgeons*, pos- "sessing *degrees* and *diplomas* which denote "that the possessors of them have gone "through a regular course of medical edu- "cation and examination, are not entitled to "practise as apothecaries, hold and deter- "mine that *chemists* and *druggists* are so "entitled under the words of the 28th clause "of the statute of the 55th GEO. III.? We 'decide, therefore, that the protection of the "chemist and druggist extends only to his "*trade* or *business*, and that it cannot secure "to or confer upon him the right of exercis- "ing the duties of a member of the medical "profession, when the Act in which that "clause is found, and the decisions of the "Courts under the Act, have withheld that "right and privilege from the highest qua- "lified members of the medical faculty." The decision, therefore, of the Court in the case of the APOTHECARIES' COMPANY *versus* GREENOUGH was perfectly consistent with the principle which had been laid down by the judges in all the antecedent cases. It is consonant with justice; it is sustained and invigorated by public opinion; it is ennobled by the vital protection which it is calculated to afford to the public health.

On reviewing the whole question, and taking into full and deliberate consideration the history of the Apothecaries' Act, there can be no doubt that the judges, by the opinion which they lately delivered in the Court of Queen's Bench, have given the most characteristic stamp of wisdom that could be afforded to their decisions in the preceding cases. Had they declared that *druggists* could exercise the duties of apothecaries, and that *physicians* and *surgeons could not*, the whole world would at once have demanded the production of the law on which such a decision was grounded. Would the proof furnished by the 28th section of the Act of 1815, have been sufficient to justify the judges in coming to such a determination? We again insert that section, and invite to it the best and earnest attention both of our professional and our non-professional readers:—

"28. Provided always, and be it further enacted, that nothing in this Act contained shall extend, or be construed to extend, to prejudice or in any way to affect *the trade or business of a chemist and druggist, in the buying, preparing, compounding, dispensing, and vending drugs, medicines, and medicinable compounds, wholesale and retail;* but all persons using or exercising the said *trade* or *business*, or who shall or may hereafter use or exercise the same, shall and may use, exercise, and carry on the same *trade* or *business* in such manner, and as fully and amply to all intents and purposes, as the same *trade* or *business* was used, exercised, or carried on by chemists and druggists before the passing of this Act."

The Courts having again and again decided that the fellows and members of colleges and physicians and surgeons were not entitled to practise as apothecaries unless they held the licence of the Worshipful Company, it was not to be expected after such decisions had *repeatedly* been made, that the above section would or could receive any other interpretation than that it afforded protection to the druggist simply and strictly in the exercise of his TRADE OF BUSINESS,—that it conferred upon him, that it secured to him, no right, privilege, immunity, or exemption as a MEDICAL PRACTITIONER, or in the exercise of the most insignificant function which legitimately and properly belongs to the profession of an apothecary.

A GENERAL MEETING of the NORTH OF ENGLAND MEDICAL ASSOCIATION was held at Durham, on Wednesday, the 13th instant, WILLIAM GREEN, Esq., of that city, president at the discussions of the meeting, and H. G. POTTER, Esq., and Dr. KNOTT in the chairs at the dinner.

At the morning assembly an ably-drawn report of the Council for the past year was read by the indefatigable hon. secretary, Mr. CARTER. It was not possible, perhaps, to introduce into it any new topics in relation to the condition of the profession, or to throw stronger light on the abuses of its government and the grievances of British medical practitioners. We do not, therefore, give it entire. It is the happiness of the independent medical press that those facts and views which it has for the last eighteen years been producing and enforcing, are now fully adopted by the profession, without expecting that additional force can be given to them now beyond that important and preponderating effect which twenty thousand supporters must afford to any arguments or events.

The report, after regretting that the "unsettled state of political affairs" in the last Parliament should have then prevented all wholesome medical legislation, observes, that—

"This circumstance, though productive of temporary disappointment, need not, perhaps, be matter of deep regret; for it was not to be expected that a Bill could pass the Legislature during the session, and the consequent delay, added to the frequent discussion of medical reform, occasioned by the promulgation of the schemes of Mr. Warburton and Mr. Hawes, and other documents, may eventually be of service in procuring a measure which may be *satisfactory to the profession at large.*"

Respecting the effects of "agitation," and the "pressure from without," the Council mention that—

"Within the last few months certain indications of a reforming spirit have been observed in some of the medical corporate bodies. The College of Physicians of London has begun the adoption of a more liberal policy than has hitherto marked its history. The exclusive privileges of graduates of the English universities has been abolished. The college has been thrown open to its members. The obnoxious appellation of 'LICENTIATE' has been abandoned, and a system of representative government introduced. Such changes afford a sufficient answer to those timid persons who excuse their own apathy in representing the efforts of reformers as useless. The council give the College of Physicians credit for their late proceedings, although forced upon them by the influence of public opinion, and of widespread discontent in the profession.

"The College of Surgeons has yet made no declaration of its intentions, although rumour states that some internal movements are going on in that institution also; and the well-known liberality of a portion of those gentlemen who are at this time members of its council, induces a belief that it cannot long remain in its present unsatisfactory condition.

"But no self-reform of any corporation will be sufficient to accomplish what is demanded in the way of reformation. The councils of the corporations must be made responsible to the members. This opinion has been acquiesced in by the Colleges of Physicians of London and Edinburgh, and by the Colleges of Surgeons of Dublin and Edinburgh.

"The council wish not to treat with disrespect, or to underrate the services of the Apothecaries' Company. Its present functions were not sought by it. They were thrust upon the Company by the culpable neglect of the Colleges of Physicians and Surgeons. But is the Apothecaries' Company the source from which medical practitioners should emanate? Its charter was granted for the purpose of protecting the public from the practices of ignorant pretenders to medical knowledge. Has it fulfilled the terms of its charter in this respect? To a very small extent. The Company, it is

said, is in possession of 1200 names of persons practising without its licence. Is it fitting that medical men should derive their authority to practise an honourable profession from a 'city guild and trading company?' And is it proper that the licensing power for the general practitioner should be held by a body which is not authorised to examine its licentiates either in surgery or midwifery?"

This is well put. Next week we may notice the topics discussed by the speakers.

CHAIR OF MEDICAL JURISPRUDENCE IN THE UNIVERSITY OF GLASGOW.

To the Editor of THE LANCET.

SIR,—The attention of the medical profession here is at present keenly directed to the filling up of the vacant chair of medical jurisprudence in the University, as you will observe from the inclosed article which appears in the *Courier* of October 19th; and which I beg, for the honour of my Alma Mater and the medical profession, you will have the goodness to insert in this week's LANCET. The candidates already mentioned are, Dr. Harry Rainy, Dr. Macgregor, Mr. Allan Burns, Dr. Laurie, and Dr. Cleland. I have the honour to be, Sir, yours truly,

A GLASGOW MEDICAL PRACTITIONER.

Glasgow, Oct. 18, 1841.

"CHAIR OF MEDICAL JURISPRUDENCE.

"It is not our intention to interfere with the disposal of this chair, but we hope the Government will consider carefully the claims of the respective candidates, and award the office to the individual, whoever he may be, that is best qualified for the discharge of its duties. There are some peculiarities about such a professorship, however, which we may as well point out.

"The general questions which come before a court of law, and which can only be decided by medical evidence, are not numerous, and might be answered as readily by one well-educated man as another; but the toxicological department is a wide and an intricate one, and requires a larger share of minute knowledge than falls to the lot of the common run of practitioners. It not only demands a perfect acquaintance with the vital laws which affect animal bodies in health, but the changes which take place in their organisation in consequence of disease, and the influences of external agents upon them in all circumstances. Without this knowledge it is impossible to understand the operation of poisons on the human frame, and equally impossible to deduce the necessary conclusions from appearances after death. Much experience and much discrimination, joined to habits of sound induction, are indispensable to success in such investigations; and these qualities, it is needless to add, rarely meet in the same individual. In selecting a professor of this branch of medical education, however, they should be looked for; and if found should have their due weight assigned to them.

"In like manner a perfect acquaintance with practical chemistry is essential. We do not mean that slight knowledge of the general principles of the science which all medical men are supposed to possess, but an absolute *bonâ fide* familiarity with the habitudes of unorganised bodies—the way in which they react one upon another—the mode of detecting their existence in compound substances, of separating them when detected, and of experimentally demonstrating their presence. This is an *art* which can only be learnt, like any other art, by the use of a man's hands, and the deliberate and long-continued exercise of his external senses. Neither lectures nor reading will teach it him; and the slightest reflection must show that no person who wants this knowledge, whatever his other gifts may be, can perform satisfactorily the duties of a toxicologist. Poisons, whether accidentally taken, or feloniously given, are generally administered in small doses; and even when it is otherwise, the quantities remaining in the body after death are, for obvious reasons, necessarily minute. How to discover these is the difficulty, for nothing short of their production will, nor ought, to satisfy the jealousy of modern justice; and the man who attempts to do so without having previously devoted a large portion of his time to chemical analysis, incurs as fearful a responsibility as it is possible to assign to any human being. The life and character of a fellow-creature depend upon his skill. He may destroy the one or blast the other by his ignorance. A very trifling blunder caused by inaptitude, or careless manipulation, may lead to the most deplorable results, and end in consequences at the very thought of which humanity shudders. Appearances in mixed fluids are always deceptive, and nothing but the power of separating the false from the true, and the unreal from the real—a power to be acquired only by practice—can enable the operator to steer his way through the shoals by which he is surrounded. We do sincerely trust, therefore, that the utmost attention will be paid by Government to this consideration, and that, if possible, the professor about to be appointed may be an individual in whose chemical attainments, as well as in whose general knowledge and respectability, the public may be able to repose entire confidence."

Elements of the Practice of Physic; presenting a View of the present State of Special Pathology and Therapeutics. By DAVID CRAIGIE, M.D., F.R.S.E., &c. &c. Vol. II. pp. 982. Edinburgh: Black. 1840.

THIS volume completes Dr. Craigie's Practice of Physic; comprising inflammations of the serous membranes, inflammations of cellular membranes and adipose tissue, inflammation of the compound organs, inflammations of the fibrous and fibro-serous membranes, hæmorrhages, and complex lesions.

In discussing the various diseases of which he treats, Dr. Craigie has followed nearly the same arrangement as he adopted in his excellent Manual of Pathological Anatomy. His classification is based upon the tissues. It is the best for a treatise of general anatomy: but we question very much the propriety of transplanting the principle of a pathological classification into a treatise on the practice of physic, from a subdivision of science in which the body is examined under a totally different point of view. Dr. Craigie's system, however, affords many interesting generalisations, and any arrangement is better than the alphabetical, in which essays on diseases are strung together according to the letter of the alphabet with which they happen to begin, as if the human understanding could discover no more affinity between diseases than between so many beads. The men who compile these physical dictionaries, should learn that the dictionary arrangement is only proper for dictionaries of words and their definitions.

Dr. Craigie's work displays much sound erudition, judgment, and practical knowledge. It is written throughout in a scientific spirit. There can be no doubt that the author is one of the first pathologists of the day, who, unlike John Hunter, at least in one respect, possesses the talent of conveying solid information in a lucid, intelligible style. We recommend the book as a safe guide to students. It does not confound the judgment by a loose compilation of uncivilised opinions; nor is the learned author infected with the mania of *accounting for everything* by imaginary theories or imaginary actions. We understand that Dr. Craigie is a candidate for the chair of pathological anatomy, now vacant in the University of Edinburgh: no one is better calculated to fill it with credit to that celebrated seat of medical instruction.

At another time we hope to notice this work at greater length: we must now be contented with quoting a short specimen, not of the many original passages it contains, but of Dr. Craigie's method of dealing with an important subject:—

"Chronic inflammation of the peritoneum differs from the acute, not only in the mode of attack but in its subsequent course. It advances by degrees, causing only occasional superficial pricking pains over the belly, without inclinations to go to stool. The tongue, especially in the morning, is slightly covered with a white fur; the thirst is considerable, and the pulse is somewhat quickened, yet it is rare to remark any exacerbation in the evening, or hectic flushes on the cheeks; the countenance, on the contrary, is languid, and the face is pale and doughy. In the early stage of the disease the patient is capable of performing his ordinary duties, and only complains after fatigue of some tightness and pricking soreness over the belly, between the flanks or haunches. The state may continue with little variation for many weeks, or even months, during which time the operation of the bowels may proceed naturally, though more frequently the patient is costive. There is no tension of the belly, as in the acute form; but the skin and abdominal muscles are, in some instances, so loose on the peritoneum as to give a sensation of their playing freely over a tight bandage below. The patient, complaining less of pain than of tightness, which is much increased by congestion of the intestines, and relieved on their evacuation.

"In the meantime the disease proceeds, till it assumes, from some accidental circumstances, the acute form, or the process of chronic inflammation becomes thoroughly established. In the latter case, as Pemberton observes, lymph is effused, which glues the intestinal folds together, and may absolutely interrupt the peristaltic motion. In the chronic disease lymph is undoubtedly deposited between contiguous folds of intestine, and in many instances the whole bowels are matted together into an agglutinated mass. This, however, is not the only effect of the process of chronic inflammation, as the disease advances, fluid of various consistence, serous, milky, sero-purulent, or purulent, is secreted, and trickles down into the lumbar hollows, and between the intestinal convolutions: as this increases the belly swells, and is distended, and in many instances distinct fluctuations may be perceived. The contained fluid is generally mixed with flakes of lymph. Pemberton remarked the tendency of chronic peritonitis to be resolved by effusing fluid and producing ascites. The truth is, that this inflam-

mation has a natural tendency to serous or sero-purulent effusion; and it is one of the most ordinary causes of the disease named dropsy of the belly (ascites).

WESTMINSTER MEDICAL SOCIETY.

Saturday, October 16, 1841.

Mr. Streeter, President.

ORIGIN OF MEDICAL SOCIETIES.—ASPHYXIA IN NEW-BORN CHILDREN.

The society commenced its sessional meetings this evening. There was a full attendance of members.

The President in his opening address made some remarks on the origin of medical societies, respecting which he stated the following facts.

The Royal Society, the parent of all other similar bodies, was originated about 1645, in the weekly meetings of a few men of science, sometimes in Dr. Goddard's lodgings in Wood-street, sometimes at a convenient place in Cheapside, and sometimes in or near Gresham College, and seemed to be the one mentioned by Mr. Boyle as the "visible and philosophical society."

About 1648-49, the company who formed these meetings divided, part removing to Oxford, where they assembled at different places, and among others at the lodgings of the Hon. Robert Boyle, while those in London continued to meet as before. About 1659, the greater part of the Oxford Society returned to London; and the two branches again reunited, and held their meetings at Gresham College. Upon the restoration in 1660, the meetings were revived, and attended by a large concourse of persons "eminent for their character and learning." They became noticed by the king, and were erected, April 22, 1663, into a corporation, consisting of a president, council, and fellows, for promoting natural knowledge. This one society included all branches of science; but with the rapid extension of knowledge, various offshoots appeared in the form of societies appropriated to the particular sciences. Among the early, if not the earliest of these, arose societies devoted to the exclusive cultivation of medicine and surgery. The oldest of these, he believed, in London, was the Society of Physicians, established in 1752, to which Dr. William Hunter* was elected in 1756. It consisted exclusively of licentiates of the College of Physicians. The volumes of its transactions were well known. The next in order of time, was the Physical Society of Guy's Hospital, which was established 1771; and nearly about the same time, and in one year, 1773, arose the Medical Society of London, formed by Dr. Lettsom; and the Lyceum Medicum Londinense, by John Hunter and Dr. Fordyce. The former held its meetings in Crane-court until 1778, when it removed to Bolt-court, where it still meets in a house presented to the society by Dr. Lettsom. The latter met in Dr. Hunter's Museum, then situated between Castle-street and the east side of Leicester-square. The same distinguished men founded the "Society for promoting Medical and Chirurgical Knowledge" in 1783, which held its meetings for more than twenty years, and finally concurred (upon the best information he, Mr. Streeter, could obtain, although some obscurity prevailed upon the exact manner of proceeding,) with the Lyceum Medicum Londinense in the forming of the Westminster Medical Society in 1809, and which had met successively in the Hunterian Theatre, Great Windmill-street, erected by Dr. William Hunter after the refusal of the Government of the day to grant a site for the erection and endowment of an anatomical museum; in Great Marlborough-street, in Sackville-street, again in Great Windmill-street, and lately in Exeter Hall.

The Medico-Chirurgical, an offshoot of the London Medical Society, had received a royal charter, but it was preceded in this privilege by the Royal Medical Society of Edinburgh in 1778: a society, probably, established and coeval with the existence of that university as a school of medicine.

Medical societies were not rivals, except in that generous rivalry and emulation of desiring to be of the greatest service to the profession and the public. The greatest men had submitted many of their productions to the criticism of societies before they published them. This was the case with Dr. William Hunter in medicine, John Hunter in surgery, Mackenzie in literature, and Dugald Stewart in philosophy. None need be ashamed of following in the footsteps of these illustrious men. The introduction of a paper before a society, might be considered as somewhat similar to what Rousseau had said of the first edition of a new work, that "it was merely circulating the proof-sheets among his friends in order to undergo their criticism and revision."

Great part of the evening was occupied by the nomination of members to fill various vacant offices, the election to which will take place at the next meeting. Mr. Snow afterwards read a paper,

ON THE RESUSCITATION OF STILL-BORN CHILDREN, AND ON ASPHYXIA IN GENERAL.

He made some remarks on the nature of respiration, and stated that the experiments of Professor Magnus on the blood, and those of Dr. Edwards on respiration in hydrogen gas, had proved that the oxygen which disappeared was absorbed into the blood, and by uniting with carbon in the capillary cir-

* Mr. Streeter mentioned, *en passant*, that *Dr. William Hunter* lodged "*at the house of Mr. Smellie, then an apothecary in Pall-Mall.*"

culation of the system formed carbonic acid, which was evolved as a gas into the air-cells of the lungs. The latent heat given out in the formation of carbonic acid, was, no doubt, the chief, if not the sole, cause of animal heat. The development of caloric throughout the animal kingdom, bore always a direct proportion to the quantity of carbonic acid produced by respiration; and the union of the oxygen and carbon in the capillary circulation at all parts of the body, better explained the uniform diffusion of warmth, than that theory which assumed the carbonic acid gas to be directly formed in the lungs.

In asphyxia, after the fœtal circulation was lain aside, the following phenomena took place as regarded the circulation, the blood at once ceased to be changed in colour whilst passing through the lungs, and venous blood circulated in the arteries; but in a very little time the blood was refused admission through the capillaries of the lungs, and accumulated in the pulmonary arteries and right side of the heart, whilst the pulmonary veins and left side of the heart became empty. The circulation was thus arrested, although the heart continued to contract for some time, and would propel the blood through the system, provided it would pass the lungs. The arrest of the circulation at the pulmonary capillaries might shorten life a few seconds, or even a minute or two; but blood which had circulated, perhaps, twice or thrice round the system, and entirely lost its arterial properties, was manifestly unable to maintain sensibility, or any of the vital functions; for new-born animals when deprived of air, lost first voluntary motion, and then every other sign of life, in the same order as adults, although the foramen ovale was open, and blood continued to be sent to the nervous centres, and the rest of the system, the action of the heart being generally the last sign of life to disappear in asphyxia.

Asphyxia arose from want of oxygen in the blood, and not from the presence of carbonic acid, since it took place in nitrogen or hydrogen gas, the same as if respiration were stopped, notwithstanding the mechanical process of breathing was performed, and the carbonic acid continued to be given off from the blood.

A consideration of great practical importance in the study of asphyxia, was the influence of the temperature of the medium in which it took place. Dr. Edwards, of Paris, by a most extensive and beautiful set of experiments, had proved that throughout the animal kingdom asphyxia was much more sudden at a high than at a low or moderate temperature; and that even cold-blooded animals, which would linger for hours deprived of oxygen, at a low temperature, died as quickly as mammalia in water, at 100 degrees Fahr., which had been deprived of its air by boiling. He found that new-born mammiferous animals died most slowly in water at 50 or 60 degrees, and that they expired much sooner as the water approached blood heat.

Dr. Edwards advised that persons in a state of suspended animation should, amongst other measures, be exposed to the cool air, and that the application of heat should be avoided, unless, indeed, just a momentary application to endeavour to arouse sensibility. The Royal Humane Society, on the other hand, directed the application of warmth in all practicable ways, not only as an auxiliary to artificial respiration, but even to commence with, if the means for the latter were not in readiness. He (Mr. S.) thought that the deleterious effects of an elevated temperature, when respiration was stopped, depended on its stimulating the capillary circulation of the system, and thus promoting the de-oxygenation of the blood, that change which was antagonistic of respiration, which ruled its extent under all circumstances, and which, in fact, constituted the necessity for having a respiration. But whatever view might be taken on this point, the fact of the undoubted influence of temperature on asphyxia proved that the application of heat ought to be avoided until respiration was thoroughly established, when it would, no doubt, be an useful auxiliary to restore sensibility and renovate the patient.

The number of children that died of asphyxia at the time of birth was very considerable, many of them were amongst the strongest and finest children, and the asphyxia took place often at the very moment of birth. The first measures generally and very properly adopted when a child was born in a state of suspended animation, were to admit the cool air to its skin, to dash a few drops of cold water on it, and use similar means to arouse sensibility, more especially that of the nerves of respiration. From the great vascularity and sensibility of the skin, and the thinness of the cuticle, great benefit might be expected from the access of air to the surface of the body. The warm-bath was sometimes had recourse to, and he had seen it completely successful in two or three instances, after the means just enumerated had failed; but this was a dangerous measure, one which if it did not succeed would quickly extinguish any possibility of recovery which might exist, as he had shown. The great object in this, as in every case of asphyxia, was to establish respiration; and if the patient could not be roused to perform natural breathing, artificial respiration must be had recourse to as quickly as possible. Several eminent authors on midwifery recommended breathing into the lungs of the child, if other means are not at hand; but not much good could be expected from a measure which would, undoubtedly, suffocate a living child; and where there was any disposition to natural breath-

ing, this would be injurious. He had tried by inhaling a full breath, and expiring immediately, to breathe out some air, containing as much oxygen and as little carbonic acid gas as possible, but could not by any ingenuity blow any air sufficiently pure to support the flame of a wax-taper. The apparatus in ordinary use for artificial respiration was the bellows; but this was liable to many objections. There was danger of injuring the texture of the lungs by over distension; there was a difficulty of expelling the air after it had been injected, and the delay occasioned by thus expelling it, rendered it impossible by means of bellows to imitate natural respiration.

Mr. Read, three years ago, laid before that society an invention for performing artificial respiration, much superior to the bellows: this, however, was not adapted for the case of new-born children; but having lately found that Mr. Read had made improvements in his instrument, and brought it to such perfection, that the use of it would supersede the natural respiration without inconvenience, he (Mr. S.) suggested that he should make a small one, adapted to the size of new-born children, on exactly the same plan. The instrument, which was on the table of the society, consisted of two syringes; one of which would pass air to the lungs by a tube adapted to the nostrils, and the other would withdraw the same quantity by another tube fitted to the mouth. The cylinders were placed side by side, and the pistons lifted and pressed down together, being both held in the same hand. When the pistons were raised, one cylinder became filled with air from the lungs, and the other with fresh air from the atmosphere, which could be warmed on its way, by passing through a metal coil, placed in a jug of hot water, and connected with the cylinder by a tube. When the pistons were again depressed this cylinder was emptied into the lungs, and the already respired air in the other was ejected into the atmosphere: in this way a constant current of air could be kept moving to and from the lungs, as in natural breathing. The introduction of warm air would be an advantage; for the objections to the application of heat, so long as respiration was stopped, did not exist when the cause of asphyxia was removed; and in introducing heat with the air to the lungs, they were only warming that blood to which they were at the same time imparting its arterial properties. The artificial respiration should be persevered in for some time, an hour at least, before the attempt should be given up in despair; and if the effort was successful, it should be continued till the child was thoroughly revived, and capable of carrying on a full and effective respiration of its own: for the secondary asphyxia, which so often came on, arose, he believed, from an efficient respiration not having been established, whence the blood remained in a badly oxygenated state, and did not rouse the nervous system to its full sensibility, but allowed it to remain in a condition, so to speak, of not truly appreciating its own want of respiration. An accoucheur could not always have an instrument with him; but it happened that the danger of asphyxia to the infant was often foreseen, from preternatural presentations and other causes, some time before the conclusion of labour. The apparatus would be useful likewise in poisoning with opium, prussic acid, or ardent spirits; in convulsive fits, and on other occasions which would suggest themselves. The syringes could be separated, and with the additional tubes provided, could be used for stomach or enema pumps. Each syringe held an ounce and a half by measure. In the case of a child which had not breathed, it would be well to use the exhausting syringe first alone, to withdraw any mucus there might be about the fauces; then, since the lungs are empty, to pass a syringe full, with the other pressing the larynx slightly upon the œsophagus at the same time, and afterwards begin with the pistons elevated, and work the two syringes together; so that respiration would be kept up without ever having the lungs either empty or distended.

The discussion was adjourned to the next meeting.

MEDICAL SOCIETY OF LONDON.

October 18, 1841.

The discussion on delirium tremens was resumed, several members taking part in the debate.

MIDDLESEX HOSPITAL.

VENEREAL ULCERS OF THE THROAT FOLLOWING CHANCRE OF THE NIPPLE, ARISING FROM SUCKLING A DISEASED CHILD.—INFECTION OF THE MOTHER'S OWN INFANT.

M. C., thirty-two years of age, a married woman and mother of six children, was received into the hospital under the care of Mr. Arnott, for a venereal affection of the throat, of which she gave the following history.

Nine months ago, whilst suckling her own child, she was persuaded to likewise suckle another infant, which was somewhat puny. She had done this for five or six days, when the nipple of one breast became so sore, that she was obliged to give up suckling the nurse-child, more especially as she began to suspect that it was diseased. A sore formed at the base of the nipple which she poulticed, and she continued to suckle her own child. At the expiration of six weeks, however, the sore became so bad, so deep and painful, that she was obliged to discontinue suckling. In

the course of the period named, her child, which had been a fine baby, began to droop, and became fretful; its mouth got sore; ulcers formed on its lips; it got thinner, and afterwards spots appeared on the feet and on the buttocks.

Some ointment was given to her, under the application of which the sore on the breast healed. After a time, however, it broke out again, and by the use of the same ointment, with decoction of sarsaparilla and iodide of potassium internally, it again healed. Subsequently her throat became affected, and has continued so.

On examination, the velum palati was found to be intensely red, with an ulcer on each tonsil, deep but not extensive; no eruption; she has lost flesh; there is a well-marked cicatrix on the base of the left nipple. At Mr. Arnott's request the child was brought to the hospital, it had sores at the angle of the mouth and several around the anus. The mother was ordered five grains, and afterwards ten grains, of blue pill at night, and five in the morning. When the gums got tender, they were kept so during the six weeks she remained in the hospital, in which time the redness of the velum had disappeared, and the tonsils had healed. She was then made an out-patient, at her own request, but recommended to take the pills for some time longer.

To the child three grains of mercury with chalk were given three times a-day; and under this treatment the sores on the lips and anus were healing when the child was last seen.

In the preceding case no mercury seems to have been administered in the first instance, and the secondary symptoms in the throat succeeded in the usual course. The same occurrence took place in the following case, the secondary affection being of the skin.

SYPHILITIC LEPRA.

D. P., aged about thirty years, was admitted May 27, 1841, under the care of Mr. Arnott, with a large superficial foul ulceration on the inner surface of the prepuce, which was everted and retracted behind the glans. The sore was an inch and a half transversely, by half an inch from before backwards; the surface was purple, in parts blackish, and without granulations. It had existed for six weeks, and was somewhat painful, so as to interfere with sleep. He had taken no medicine, and had treated the sore with soap and water. To have ten grains of Dover's powder every night. Use equal parts of liquor opii sedativus and distilled water as a lotion to the affected part.

June 11. The sore has been for some days clean, florid, and granulating. To-day the patient directed attention to spots of well-marked scaly eruption on the shoulder. Ordered to take ten grains of blue pill every night, and five grains every morning.

24. The sore heals kindly. Gums slightly affected.

July 15. Sore quite healed for some days; the eruption has disappeared; the mouth has been so sore that the pills were discontinued: to be resumed for a fortnight, in quantity to keep the gums tender.

LETTER FROM MR. BLYTHMAN.

To the Editor of THE LANCET.

SIR,—My attention has been directed by a distant medical friend to a case of rupture of the uterus purporting to be reported by me, in your valuable periodical of the 2nd of October instant. I have to request that you will take the earliest possible opportunity of informing your numerous readers that the details of that case are *maliciously false*, and that the dastardly scoundrel who has attached my name to it has committed forgery. A case of rupture of the uterus unfortunately did occur in my practice about the time and at the place mentioned, but that I should so far stultify myself as to publish the case in the manner related in your periodical, is an idea that would have emanated from no other brain but that of your worthy correspondent. The statement regarding the proceedings and practice of the medical friends whom I called in upon the melancholy occasion, as well as the appearances which are said to have been presented at the post-mortem examination, are so utterly devoid of truth, that to give a detailed contradiction of them would occupy too much space in your valuable pages. I shall therefore conclude with sincerely thanking you for the promise you have given me to afford me all the means in your power to drag to light and punish, as he deserves, the miscreant who could thus dastardly borrow my name to endeavour to blast my future prospects in life. I cannot help adding, that the scoundrel who has done *this* is below the level of a cut-purse or a highwayman. Yours respectfully,

ROBERT OTTER BLYTHMAN.

Swinton, near Rotherham, Oct. 16, 1841.

LUNATIC ASYLUMS' INQUIRY.

To the Editor of THE LANCET.

SIR,—The last effusion of Mr. Bodington clearly demonstrates the absence of all restraint at Driffold House, and may induce an opinion that gentle coercion would not be misplaced in that establishment; at least so far as its superintendent is concerned. Self-command is the first lesson to be learned by all who assume to command others, and if the temper, style, and habits, of Mr. Bodington afford a fair specimen of the general characteristics of the proprietors of private lunatic asylums, which Heaven forfend! it would seem that a searching inquiry is requisite,

not only into the general management of the institutions, but into the qualifications of the superintendents. I should not have noticed Mr. Bodington's last angry ebullition but for this passage in it:—

"I have affixed my name to these letters, being aware that I should be compelled to make statements respecting the management of some county asylums for which I ought to stand openly responsible in my defence of private establishments."

Mr. Bodington may rest satisfied, that whenever an inquiry is made into the management of lunatic asylums, it will comprehend *public* as well as *private* establishments, and the "LOOKER-ON" will take care that he has ample opportunity of making good his charges before the appointed tribunal, not only as regards the four asylums from whom he says, patients have been admitted into Driffold House, but as regards any other public institutions which he may think fit to attack. The object of the "LOOKER-ON" is the correction of abuses. It is immaterial to him what is the nature of the institution in which they may exist. I am, Sir, yours obediently,

THE LOOKER-ON.

October 19, 1841.

DR. HITCH'S REPLY TO "A LOOKER-ON."

To the Editor of THE LANCET.

SIR,—I beg a space in your Journal to assure the "LOOKER-ON" that he has misunderstood me. I do *not* "hold him in contempt;" I entertain for him (be he who he may) very different feelings: and if I differ from him in opinion, or object to the mode he pursues in carrying out his best intentions, I neither possess nor desire to excite disrespect towards him.

Whilst such are my *private* feelings, it must appear strange that I am made to use language *in public* susceptible of opposite construction. Another perusal of the portion of my letter published by Mr. Bodington, will make it apparent to the "LOOKER-ON" that I had no such intention: in that it will be seen that I furnished Mr. B. *with facts*, which I placed at his disposal. In publishing more than these Mr. B. has misunderstood me. It is true, I added a general opinion of a practice (*now not uncommon*), and I expressed an opinion of an individual; but coupling these with a disavowal of controversial intention, it is clear I did not design they should be further made use of—I meant them for Mr. B. alone: they are, however, now before the world, and I do not shrink from them.

On the subject of "breaking a lance" with the "LOOKER-ON," I have more than one word to say. I object on principle to controversial correspondence on medical subjects; I have never known such productive of much good, and have seen much evil arise out of them: the opponents rarely contrive to understand each other to the end of the controversy, and the subject becomes lost in the disputants themselves. I object, too, to be drawn out in public by one who, for reasons which may please himself, gives that public no security that he is entitled to make the demand: but if the "LOOKER-ON" desires to draw from me (*for the public use*) any portion of the experience which may have been acquired in this institution; whether to exhibit the means of its success or the cause of its failures; or to compare the *humane* treatment of *another* asylum with that which he may deem *less humane* in this, let him but raise his beaver *to me*, that *I may* know to what good knight our cause is indebted, and he shall find me always ready to respond to his call.

It is bad taste to smile at the morbid sensibilities of another under any circumstances, but I cannot withhold one at the distaste evinced by the "LOOKER-ON" at the mention of our provincial and uncouth weapon; he must, however, allow me to remind him, that if he does not like to play with *this*, the hard blows he often deals are not given with a much lighter or softer instrument: but I pledge myself that should we hereafter become more communicative, neither his metropolitan eyes nor ears shall again be offended by a display of such a vulgarism.

I am, Sir, your obedient servant,

JAMES HITCH, M.D.,
Resident Physician, Gloucester Lunatic Asylum.

Gloucester, Oct. 11, 1841.

BOOKS RECEIVED.

Researches into the Causes, Nature, and Treatment of the more prevalent Diseases of India, and of Warm Climates generally. By James Annesley, F.R.S., F.S.A. Second Edition. London: Longmans and Co. 1841. 8vo. Pp. 606.

On the Remote Cause of Epidemic Diseases. By John Parkin. London: Hatchard and Son. 1841. 8vo. Pp. 198.

TO CORRESPONDENTS.

St. Marylebone Infirmary.—After the publication of the letter of Dr. Clendinning and that contained in the present number of our Journal, it will be unnecessary to insert Mr. Norton's communication.

Quæsitor had better await the publication of the entire paper, after which, if he be not fully informed, we shall be glad to reply to any question that he may offer. Another portion will complete it.

J. T. C.—Manchester-square, London.

Mr. Daris.—Oleum animale is an empyreumatic oil obtained by the decomposition of animal substances by heat. It is obtained in the manufacture of ivory black.